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PROFESSOR CAYLEY, President, in the Chair.

J. M'Landsborough, Esq., Bradford, and
E. B. Knobel, Esq., Burton-on-Trent,

were balloted for and duly elected Fellows of the Society.

Copy of a Letter from the Astronomer Royal to the Secretary of the Admiralty expressing his Views on certain Articles which had appeared in the Public Newspapers in regard to the approaching Transit of Venus.

(*Communicated by the Lords Commissioners of the Admiralty, and printed by order of the Council.*)

I have the honour to acknowledge your letter of February 14th, calling my attention to articles which have appeared in the *Spectator* newspaper of February 8th, and the *Times* of February 13th, and requesting my views on these for the information of the Lords Commissioners of the Admiralty. I have procured the papers in question, and also the *Times* of February 20th, which contains articles of opposite characters bearing on the same subject; and have also received a paper by Dr. Oppolzer of Vienna, of very elaborate character, scarcely known in this country. I have the honour now to offer my remarks upon the whole subject.

2. The English papers are moderate and courteous in character, though distinct in their meaning. They are based entirely

upon investigations by Mr. R. A. Proctor published in the *Monthly Notices*, especially in the *Notice* of June 11th, 1869. These investigations are illustrated by tables and maps of great clearness and unquestionable value ; and to these, mainly, I shall refer in the following remarks. I shall, however, also extract some numbers from the calculations made by the Superintendent of the *Nautical Almanac*, and published in the *Nautical Almanac* for 1874 ; and some notes from the paper of Dr. Oppolzer to which I have alluded.

3. Remarking that the proposal to make observations on the Antarctic Continent is essentially connected (as I shall shortly show) with the idea that efficient observations may be made in the extreme north of Asia, I will first point out that this matter entered into my consideration in my original communication to the Royal Astronomical Society in 1857. In speaking of "the application of the method of difference of duration of transit" (called "Halley's Method" in the newspapers) "to the transit of 1874," I said, "The most northerly stations are to be found in Siberia, Tartary, and Thibet (which will scarcely be visited by Astronomers in December), on the coasts of China, and in North British India." And, remarking that the whole discussion is founded on the assumption by Mr. Proctor that comparisons of duration of transit at different places possess a high value, I state that in 1869 M. Puiseux had indicated this ; that in the *Monthly Notices*, March 12th, 1869, I answered M. Puiseux, showing that I had already considered his suggested stations, and also that a ratio of probable errors to which he had not adverted must be taken into consideration. I believe that the idea has not again been promulgated on the Continent. I advert to these historical points for the purpose of showing that the matter has been sufficiently present to my mind. Subsequently Mr. Proctor took up the question, straining to the utmost the idea of separating as far as possible the northern and southern stations, and considering nothing but their geometrical relation.

4. I may here conveniently cite some passages (translated) from Dr. Oppolzer's paper, page 75 : "In Halley's method in general, the longitude of the place of observation needs only approximately to be known, in order to determine the value of the parallax from the time of duration ; and this was in the eighteenth century to be regarded as an especially great advantage, as the accurate determination of longitudes was then an almost insoluble problem ; but in the present very perfect knowledge of the Moon's motion, there will be no very great difficulty in obtaining satisfactory determinations of the longitude. The application of Halley's method possesses therefore in the present day no special advantage. If the longitude is determined with certainty, it is of more advantage to the accuracy of the result not to use the observations as durations, but to treat them as made for the application of Delisle's method [the comparison of absolute times at different stations]."

5. My own preparations have been going on entirely in the same spirit as that which influenced Dr. Oppolzer in writing these remarks. The labour which I contemplate as to be employed in determining the local longitudes very far exceeds that for the mere observation of the transit. In a stay of three months I hope to be able to obtain at each station 30 meridional transits of the Moon, and 120 extrameridional transits, vertical or horizontal; and I do not doubt that the longitude obtained will be certain within 1^s of time.

6. Assuming (as Mr. Proctor appears to have assumed, and with my assent thereto) the probable error of clock observation of an ingress or egress as $4^s{\cdot}28$, then the probable error of absolute time in seconds is $\sqrt{\{(4{\cdot}28)^2 + 1\}}$,* and the probable error in the comparison of absolute times at two stations will be $\sqrt{2} \times \sqrt{\{(4{\cdot}28)^2 + 1\}}$. This is the probable error for Delisle's method. When both ingress and egress are observed at one station, the probable error of each is 4·28, the probable error of the interval is $\sqrt{2} \times 4{\cdot}28$, and the probable error of the comparison of the intervals thus observed at two stations is $2 \times 4{\cdot}28$. This is the probable error for Halley's method. The proportion or $\frac{\text{Halley's}}{\text{Delisle's}}$ is 1·379. The demerit of a comparison of observations depends on the proportion

$$\frac{\text{Probable Error}}{\text{Difference attributable to Parallax}}$$

Therefore, in order to ascertain whether a comparison in Halley's method is as good as one in Delisle's method, we must find whether the parallax-difference in Halley's method is greater than that in Delisle's method in the proportion of 1 : 1·379. And taking Mr. Proctor's selection of a fundamental Delisle's comparison (Woahoo—Crozet Island) at $23^m{\cdot}8$, and remarking that $23{\cdot}8 \times 1{\cdot}379 = 32{\cdot}8$, we finally arrive at this simple criterion, that if the parallactic difference of duration in Halley's method in any special comparison of observations is greater than $32^m{\cdot}8$, that comparison is more valuable than one in Delisle's method; if less than $32^m{\cdot}8$, it is less valuable than one in Delisle's method.

7. I will now exhibit the application of this criterion to three northern and three southern stations :—

Station.	Parallactic Effect.	Comparison with Enderby.	Comparison with Nertschinsk.
	m	m	m
Nertschinsk	15·6	35·9	
Tientsin	13·4	33·7	
Pekin	12·9	33·2	

* In Mr. Proctor's formula, *Monthly Notices*, 1869, June 11, page 315, the factor $\frac{1}{2}$ is omitted by a printer's error. The computed numbers appear to be correct.

Station.	Parallactic Effect.	Comparison with Enderby.	Comparison with Nertschinsk.
	m	m	m
Enderby	20·3	..	35·9
Crozet	16·8	..	32·4
Kerguelen	16·6	..	32·2

The combination of Nertschinsk and Enderby gives a number greater than the criterion ; and therefore, if there were no other difficulties in the way, it would be well to combine these two stations. But the combination of Nertschinsk with Crozet or Kerguelen gives a number below the criterion ; and therefore, if Enderby is not secured, the observation at Nertschinsk possesses no special value of the kind considered. The combination of Enderby with Tientsin or Pekin gives a number scarcely above the criterion, and therefore, if Nertschinsk is not secured, the observation at Enderby scarcely possesses any special value.

8. I will now advert to the local circumstances. A few of the following numbers are taken roughly from Mr. Proctor's maps, but none are seriously in error.

Station.	Latitude.	Sun's Elevation a Ingress and Egress	
	°	°	°
Nertschinsk	51 +	12	10
Tientsin	39	23	22
Pekin	40	22	21
Enderby	(66)	(20)	(40)
Crozet	(46)	(11)	(50)
Kerguelen	49	28	59

Nertschinsk is a station in Siberia, in high latitude, nearly 1000 miles from the nearest sea. I presume that its climate is truly continental. At St. Petersburg, in the winter, the Sun sometimes is not seen for several weeks together; I suppose that the same may happen at Nertschinsk. The Sun's elevation at the observations will be rather small. I doubt greatly the probability that any observations can be made there.

Of Enderby's Land very little is known. It is certain that an expedition to that region must be exposed to long confinement and great severities, affecting even the metallic instruments.

9. On a review of the whole case, I decline to recommend that an expedition be sent to Enderby Land, or to any station in the Antarctic Continent.

10. In one of the papers to which my attention was called, it was suggested that, referring to the belief that Lord Lindsay proposes to make observations at the Mauritius, the Rodriguez observations should be abandoned, and a ship thus set at liberty for the South Continent. I very highly respect Lord Lindsay's spirit of enterprise, but there are abundant reasons for refusing to abandon a Government expedition in the hope that a private

Astronomer will do something equivalent. I add that the position of Mauritius is inferior to that of Rodriguez.

11. It seems to be conceived by the writers of the papers that no attention has been given to India. For myself I can say that it has been duly considered, but that, in reference to the original proposal of relying on eye-observations, I saw no reason for establishing a station there, and in particular I objected to a proposed substitution of Peshawur for Alexandria. The introduction of photography has introduced new geometrical considerations, principally pointed out by Mr. Proctor; and I have endeavoured to carry out the establishment of a photographic station in India. The matter is now in a stage in which I have no control or official information.

12. The only extension of the original plan to which I look as a contingency is this. My scheme was drawn up, and action was taken on it, before the outbreak of the war between France and Germany. In proposing that Great Britain should take up a station in the Sandwich Islands, I expressly recorded my hope that France would equip a station at the Marquesas. This hope may now be frustrated, although I know so well the noble spirit of that nation in all matters relating to science that I can scarcely entertain the thought. But I am not aware that any positive arrangement has yet been made, either by the French or the American Governments, for filling up the gap in the Pacific. At present, that is the weak part of established plans. Should the deficiency continue to exist, some effort might well be made to supply it.

Royal Observatory, Greenwich,
1873, *February* 21.

Remarks on a Letter from the Astronomer Royal to the Secretary of the Admiralty, on the subject of the approaching Transits of Venus. By Richard A. Proctor, B.A., Cambridge.

I take the articles of this letter *seriatim.*

1 and 2 need no comment other than a recognition on my part of the courteous tone in which they are expressed.

3. I shall presently show that efficient observations at high northern stations are not necessary in order to render observations by Halley's method superior to the proposed observations by Delisle's. I am at a loss to understand how the question can be said to have been sufficiently taken into consideration in the Astronomer Royal's original communication to the Royal Astronomical Society in 1857. I would refer to that communication itself, and to my remarks upon it and quotations from it elsewhere in the present Number of the *Notices.* The mere circumstance that the estimated epochs of ingress and egress were nearly an hour in error would suffice of itself to show that that communica-

tion did not and could not indicate the actual circumstances of the transit of 1874. It is a mere accident that the places indicated fall on parts of the somewhat extensive regions there named by the Astronomer Royal; but no details as to time-differences or solar elevation could have been deduced from the rough investigation then made, nor as a matter of fact is a single detail of the kind mentioned in that communication. Nevertheless, as it was the first, so also it was the last communication in which the Astronomer Royal made any definite statements respecting the value of Halley's method in 1874. These remarks relate also to the Astronomer Royal's reply to M. Puiseux in the *Monthly Notices* for March 1869, which I found myself at the time (as I find myself now) unable to reconcile with the paper of 1857. Whether I am mistaken or not in my interpretation of the latter paper can be ascertained by a study of its contents so far as they relate to the transits of 1874 and 1882.

I conceive that a careful comparison of the Astronomer Royal's discussions of the transit of 1882 with my discussion of the transit of 1874, considering both with special reference to the application of Halley's method, will afford ample evidence that I have not led the way in "straining to the utmost the idea of separating as far as possible the northern and southern stations, and considering nothing but their geometrical relation."

4. Premising that Dr. Oppolzer's paper, assuming it to have the Astronomer Royal's assent, has led to a marked change of opinion on his part as to the advantages of Halley's method, I would remark that that paper strengthens all the reasons which have been urged by myself for selecting stations where the whole transit can be observed. For if, besides the use which can be made of the observations of duration at such places, the time observations can be used for Delisle's method also, such stations have an enhanced value. This applies with special force to Antarctic stations for observing the transit, since at every such station advantageous for applying Halley's method, Delisle's method can be applied advantageously *both* at ingress and egress, and *at one or other* more advantageously than at any of the selected southern stations for observations by this method. This remark relates also to stations which are not within the Antarctic circle, and can undoubtedly be reached in December; and it is especially to be noted, that there would be no occasion to make observations for determining the longitude, until the actual observation of the transit had been successfully made. If the weather were unfavourable during the transit, there would be an end of the matter. If the weather were favourable, astronomers could thereafter, at their leisure or convenience, determine the longitude of stations where observations had thus been successfully made. Again the remark applies with special force to the North Indian stations suitable for observing the retarded egress. At Delhi, for example, while the circumstances for observing this special phase are superior (when solar elevation is taken into account) to those at the selected station, Alexandria, the whole transit will also be

observable, whereas at Alexandria only the end of the transit will be seen, and that under conditions by no means favourable.

5. This article illustrates strikingly the superior simplicity of Halley's method. Granting Halley's method to be only equal to Delisle's in any given case, it nevertheless must be far less costly if a three months' stay is regarded as necessary for determining the longitude in the application of the latter method. This remark would of course not apply to the suggested occupation of Possession Island. But it is to be remembered that Antarctic exploration, and particularly an Antarctic wintering expedition, would have very great scientific interest apart from astronomical considerations. In proof of this I need only refer to the passages quoted in my accompanying paper from the Astronomer Royal's communication in the *Monthly Notices* for May 1865.

6. I must remark that in assuming $4^s{\cdot}28$ as the probable error of clock observation of ingress or egress I was adopting, without assenting to it, an extreme estimate. I should regard the probable error of such observations made as during the transit of 1769 to be about $4\frac{1}{4}^s$; but in the observations to be made during the coming transit, guided as they will be by the experience which has been obtained in these matters, I should consider 3^s an ample estimate of the probable error. Adopting this estimate instead of Mr. Stone's estimate (strained to a maximum, I conceive) we obtain a proportion much more favourable to my case.

But *I do not press this point, simply because it is a matter of opinion.* I note only that Mr. Stone's estimate seems to me excessive.

I challenge directly, however, the Astronomer Royal's adoption of my own selection (Woahoo and Crozet Island) as a fundamental Delisle's comparison. I took purposely an extreme case in favour of Delisle's method; and if that extreme case is admitted, so also must the extreme cases for Halley's method. But if, in testing Halley's method, we are to take into account other considerations than geometrical ones, so also must we in testing Delisle's method. I apprehend that the comparison must, in fairness, be made with the Astronomer Royal's proposed arrangements, not with a scheme which was only suggested hypothetically.

7. I will exhibit the application of the Astronomer Royal's criterion (reserving my assent thereto) to his selected stations:

Pairs of Stations.	Phase to be Observed.	Parallactic Effect.	Time-Difference. ×1·379.
		m	m
Woahoo	Ingress	Acc. 11·2	29·1
Rodriguez		Ret. 9·9	
Auckland,* N.Z.	Egress	Acc. 8·5	28·0
Orsk (Russian Station) ..		Ret. 11·8	
Auckland, N.Z.	Egress	Acc. 8·5	25·5
Alexandria		Ret. 10·0	

* I find this station in the announced list of places to be occupied by England; but if I remember rightly there has been some change. In default of the

It is with these values, and not with the value resulting from the consideration of a station which is not, in point of fact, to be occupied, that comparison should be made. Let it suffice to say on this point that taking the least favourable cases in the Astronomer Royal's table,—viz. Pekin and Kerguelen's Island, we obtain as the difference of duration

$$12^m{\cdot}9 + 16^m{\cdot}6 = 29^m{\cdot}5.$$

which is better than the best of the above values, obtained by applying to the best of the stations actually selected, the Astronomer Royal's extreme criterion.

For further evidence on this point see the table which follows this paper.

As to local circumstances, I may point out that they affect the application of both the methods. Cloudy weather at Woahoo in December is a contingency far too probable to be overlooked. Observations of Orsk, Omsk, and Tobolsk, are not unlikely to be lost through bad weather. Much must inevitably be risked, whatever plans be adopted ; and as my suggestions involve a greater spreading of the observers than the Astronomer Royal contemplates (since America, Germany, and France, are likely to congregate around the very regions marked out for Great Britain), I conceive that on the whole (and apart from all other consideration) the chances of success would be increased by the suggested change of plan.

I dwell with special force upon the fact that the effective use of Halley's method would render it necessary to occupy two regions, one lying between the northern regions for observing accelerated ingress and retarded egress, the other lying between the southern regions for observing retarded ingress and accelerated egress. This I take to be a most important point ; for the chances of a successful result cannot but be reduced if these regions of vantage are left unoccupied.

9. Compare the passages cited from the Astronomer Royal's communications to the Society in 1857, 1864, 1865, and 1868, in my accompanying paper.

10. Relates to a matter respecting which it would be improper for me to express an opinion.

11. I venture to quote, in full, from the *Monthly Notices* for December 1869, the Astronomer Royal's remarks on the stations suitable for observing the egress as retarded by parallax. I cannot find any evidence in these remarks (his last public remarks on the subject) to prove that the North Indian stations suitable for observing this phase (the only phase for which North Indian stations are in question at all) have been "duly considered :"—

"The stations which are favourable for this observation are almost entirely on Russian and Turkish territories. At none of them is the factor less than 0·84 ; and we have, therefore, only to consider the elevation of the Sun, leaving to the national govern-

actually selected station, I adopt the station originally proposed. Whatever difference there may be in the acceleration will be slight.

ments to estimate the facilities or difficulties depending on the locality, the climate, or the season. Any station either to the east or to the west of the Lower Caspian will have the Sun well elevated. Omsk, Orsk (whose longitude has been determined with peculiar care), Astrakhan, Erzeroum, Aleppo, Smyrna, and Alexandria, have the Sun sufficiently high. At Tobolsk, Perm, Kazan, Kharkov, Odessa, Constantinople, and Athens, the Sun will be rather low, and at Moscow it will be on the horizon. We may with the utmost confidence leave the selection of the stations, the determination of longitude, and the observation of the phenomenon, to our Russian friends. One station, however, ought specially to be considered as being, for this purpose, in British hands, namely, Alexandria. It appears not improbable that we may soon have very direct telegraphic communication with Alexandria ; but failing this, I trust that no efforts will be wanting to determine accurately its longitude ; a longitude which was, in the survey of Admiral Smyth, and which always must be, the zero of longitude in the Levant. This being ascertained, Alexandria would probably be the best of all the stations for observation of the Retarded Egress."

I myself only indicated Peshawur as marking a suitable part of India. Delhi and other conveniently accessible stations would serve equally well.

12. Relates to considerations respecting which I have no opinion to offer, beyond the expression of my belief (justified by events which have recently occurred), that the share taken by America in the work of observing the coming transit will be a large and important one.

I may remark, in conclusion, that as respects wintering at Possession Island, I have no opinion of my own to maintain. I have simply adopted the statements of the most experienced authorities on the subject. When the Astronomer Royal was earnest in endeavouring to secure observations by Halley's method in 1882, he accepted the favourable statements of those authorities readily, or rather, even eagerly; now that Halley's method is being discountenanced, those favourable statements are lightly regarded. For myself I am content, in a matter in which I have not the least experience, to take the opinions of those who are understood to have great experience.* If Admirals Richards and Ommanney, and Commander Davis now consider that they were mistaken in what they said in 1868, so be it,—I accept their present opinion. It does not affect my argument. Many stations remain which are certainly accessible early in December, and would give better results by Halley's method than even Sir George Airy expects to obtain by Delisle's.

* I pointed out, however, in my *Sun* (and I believe I was the first to remind the general public of the fact) that Possession Island, owing to the enormous quantities of guano, would be a disagreeable if not dangerous wintering station. Still I supposed that the fact had been in the remembrance of Commander Davis, when he decided that it was possible to winter at Possession Island.

A Table showing the Difference in the Duration of the Transit of Venus in 1874, *as observed at Fourteen Northern and Seventeen Southern Stations; and serving to indicate the Value of Halley's Method in these cases, as compared with Delisle's (applied at selected Stations, and judged by the extreme criterion indicated by Mr. Stone and accepted by the Astronomer Royal).* By Richard A. Proctor, B.A.

Northern Stations.	Enderby Land.	Kemp Island.	Possession Island.	Adélie Land.	Sabrina Land.	Crozet Island.	Macdonald Island.	Kerguelen Land.	Emerald Island.	Macquarie Land.	Campbell Island.	Royal Co. Island.	Auckland Island.	Hobart Town.	Canterbury (N.Z.) and Bourbon Island.	Melbourne and Mauritius.	Adelaide and Rodriguez.
	Southern Stations.																
	m	m	m	m	m	m	m	m	m	m	m	m	m	m	m	m	m
Nertchinsk ..	35·9	34·2	33·0	33·0	33·0	32·4	32·3	32·2	29·3	29·1	28·7	28·6	28·0	26·0	25·7	24·4	23·9
Tsitsikar	34·6	32·9	31·7	31·7	31·7	31·1	31·0	30·9	28·0	27·8	27·4	27·3	26·7	24·7	24·4	23·1	22·6
Kirin Oula ..	34·4	32·7	31·5	31·5	31·5	30·9	30·8	30·7	27·8	27·6	27·2	27·1	26·5	24·5	24·2	22·9	22·4
Tien-tsin	33·7	32·0	30·8	30·8	30·8	30·2	30·1	30·0	27·1	26·9	26·5	26·4	25·8	23·8	23·5	22·2	21·7
Jeddo	33·5	31·8	30·6	30·6	30·6	30·0	29·9	29·8	26·9	26·7	26·3	26·2	25·6	23·6	23·3	22·0	21·5
Pekin	33·2	31·5	30·3	30·3	30·3	29·7	29·6	29·5	26·6	26·4	26·0	25·9	25·3	23·3	23·0	21·7	21·2
Tchefoo	32·7	31·0	29·8	29·8	29·8	29·2	29·1	29·0	26·1	25·9	25·5	25·4	24·8	22·8	22·5	21·2	20·7
Nagasaki	32·2	30·5	29·3	29·3	29·3	28·7	28·6	28·5	25·6	25·4	25·0	24·9	24·3	22·3	22·0	20·7	20·2
Bonin Is.	31·7	30·0	28·8	28·8	28·8	28·2	28·1	28·0	25·1	24·9	24·5	24·4	23·8	21·8	21·5	20·2	19·7
Nankin	31·5	29·8	28·6	28·6	28·6	28·0	27·9	27·8	24·9	24·7	24·3	24·2	23·6	21·6	21·3	20·0	19·5
Canton	28·5	26·8	25·6	25·6	25·6	25·0	24·9	24·8	21·9	21·7	21·3	21·2	20·6	18·6	18·3	17·0	16·5
Hongkong ..	28·4	26·7	25·5	25·5	25·5	24·9	24·8	24·7	21·8	21·6	21·2	21·1	20·5	18·5	18·2	16·9	16·4
Peshawur	28·3	26·6	25·4	25·4	25·4	24·8	24·7	24·6	21·7	21·5	21·1	21·0	20·4	18·4	18·1	16·8	16·3
Delhi	27·2	25·5	24·3	24·3	24·3	23·7	23·6	23·5	20·6	20·4	20·0	19·9	19·3	17·3	17·0	15·7	15·2*

Note.—The time-difference at Auckland and Alexandria, multiplied by 1·379, amounts to $25^{m}\cdot5$; and all values exceeding this in the above Table, that is, all above and to the left of the double zigzag, indicate the superiority of Halley's method at the corresponding pair of stations to this selected instance of the application of Delisle's method. In like manner all values above $28^{m}\cdot0$, or above and to the left of the single heavy zigzag, indicate the superiority of Halley's method over Delisle's, applied at Auckland and Omsk. All values above $29^{m}\cdot1$, or to the left of the single light zigzag indicate superiority over Delisle's method applied at Waohoo and Rodriguez.

* The maximum observable difference in the transit of 1882 will be 28^{m} (only observable under very unfavourable conditions, and by reaching an Antarctic station at Sabrina Land). It will be observed that the least of the differences in the above Table $[27^{m}\cdot2$ exceeds the half of 28^{m}. I venture to point out that, comparing this

The Transit of Venus in 1874. By Richard A. Proctor, B.A., Cambridge.

It will be in the remembrance of most of the Fellows of this Society that four years ago a paper of mine was read in which the circumstances of the transit of 1874 were subjected to a more careful and detailed examination than had till then (to the best of my knowledge) been applied to the subject. Towards the end of that paper * I pointed to certain conclusions respecting the application of Halley's method during the transit, which were certainly new at that time, though they have since been abundantly confirmed in many different quarters, notably in the pages of the *Nautical Almanac* for 1874. Two months later, or in May 1869, I read a paper in which these conclusions were further advocated, and illustrated by a series of projections.† Thereafter, with the exception of two or three short notes to meet objections which had been raised by Mr. Stone to points of detail, I have not, at any of our meetings, or in Council, touched further upon the subject of the approaching transits.‡

I trust it will not be thought by the Society that I have exhibited any undue impatience. My fear is, indeed, that hereafter the examination of all the circumstances may lead to the impression that I have been remiss (holding the opinions which I entertain on this subject) in leaving the matter to so late an epoch. This, however, will be regarded, I conceive, as a fault on the right side, more particularly when I mention that, as editor of the Proceedings (during Professor Cayley's Presidentship) I have

* I feel bound to dwell upon the fact that the paper was commenced, and continued (*pari passu* with the calculations whose results it presents) up to the portion referred to in the title as "An Addendum," before I had any notion that the results at which I should arrive would be different from those indicated from the year 1857 to December 1868, by the Astronomer Royal. Any one who will examine the essay carefully will recognise abundant evidence of this fact. It was somewhat hastily and quite mistakenly assumed that the paper was "an attack from beginning to end" on the Astronomer Royal's essays.

† In this paper, the series of results which I controverted are simply described as those obtained by a certain process, and they are tabulated under the letter A.; Puiseux's results, which are much more near to exactness, are tabulated under the letter B.; my own under the letter C.

‡ Certain recent events compel me to add two facts relating to the history of this matter :—

Last midsummer I wrote to the Astronomer Royal urging (strongly, but courteously) the points which seemed to me (and still seem) most important. I waited half a year. Then I wrote again, describing what I proposed to do in the present paper and illustrative chart. The construction of this chart did not occur to me *after* the meeting of the Council in January last, as it has pleased some to state. On the contrary, the chart (completely ready for the engraver) was submitted to inspection at that very meeting, as the minutes will show; and its publication in our Proceedings was authorised by a unanimous vote of the members of Council then present.

felt some degree of delicacy in touching upon a matter which had formerly been a subject of discussion between myself and the late First Assistant at the Greenwich Observatory.

There is now no time for further delay, at least as respects the part of the subject which I propose chiefly to consider — the advisability, namely, of endeavouring to secure Government assistance for an expedition to the Antarctic regions with the object of applying Halley's method to the transit of December 9, 1874.

Moreover, it is absolutely necessary for the due enforcement of my views (now that time so presses) that I should indicate precisely how and where the mistakes arose which led to the adoption of views altogether different. Because if I fail to do this, the result will inevitably be that many persons will suppose me to be merely advocating a certain opinion (and that against the leading authority in all such matters), whereas in reality the question is one of easily ascertainable facts. In saying this I speak from experience. Four years ago, I published my views in our *Monthly Notices*, without explaining where and how the views which I opposed had had their origin ; and I find that quite a large proportion of those who read my papers judged that I merely differed from Sir George Airy on a matter of opinion, not on matters of fact (mathematically testable).

What I propose to prove, in what follows, is, that certain advantageous circumstances, which had been supposed to exist only in the case of the transit of 1882, exist in a greater degree in the case of the earlier transit ; that difficulties which render the observations of Halley's method inapplicable in 1882 do not exist in 1874 ; and, lastly, that the statements (I may say the earnest appeals) made by the Astronomer Royal in the case of the transit of 1882, pledge this country (because made by its official astronomical representative) to the duty of undertaking an expedition to view the transit of 1874 (now that its circumstances are known) from some Antarctic station or stations.

Let it be premised that the reasoning by which, in 1857, it seemed to be demonstrated that Halley's method is inapplicable in 1874, is sound in its general bearing. It breaks down only when tested by details.

Let Fig. 1 represent the face of the Earth as supposed to be seen from the Sun during a December transit, such as either of the transits now approaching. Now the Earth during the transit is moving from right to left, or in the direction shown by the long arrow. Her rotation shifts points on her surface in the way shown by the small arrows on the latitude parallels, the shift due to this cause being greatest on the equator. This motion manifestly takes place in a sense adverse to that of the Earth's motion of revolution, everywhere except at stations on the shaded lune of the disk. Now *Venus* transits with the excess of her motion of revolution over the Earth's ; and anything which tends

to reduce the effects of the Earth's motion of revolution, increases the excess of *Venus's* motion,—or, in other words, hastens *Venus* in her transit. So that at every point of the unshaded portion of the disk in Fig. 1 *Venus* is hastened, more or less, by the effects

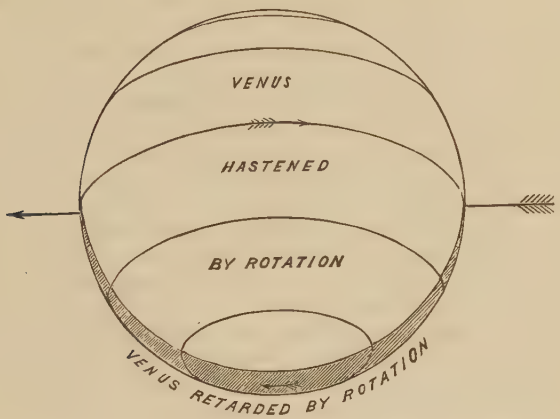


Fig. 1.

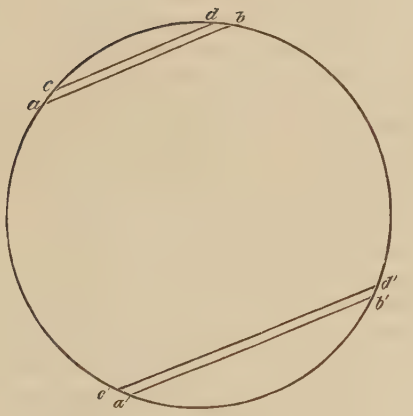


Fig. 2.

due to the Earth's rotation. On the contrary, at every point on the shaded portion of the disk *Venus* is retarded in her transit.

Now let it be noticed that these circumstances affect diversely

the two transits of such a pair as we are now awaiting. If fig. 2 represents the Sun's disk, the north point being uppermost, then the lines $a\,b$, $c\,d$, will represent chords of transit in 1874 ($a\,b$ being the chord for a northern, $c\,d$ being the chord for a southern observer); and $a'\,b'$, $c'\,d'$ will represent chords of transit in 1882 ($a'\,b'$ being the chord for a northern, $c'\,d'$ the chord for a southern station).

Now it is manifest that in 1874 the conditions affecting the duration of the transit as seen at a northern station are adverse. The chord $a\,b$ is longer, owing to the northerly latitude of the observer; but *Venus* is hastened on her course, and therefore the lengthening is not so great as it otherwise would be. We have then one favourable and one unfavourable condition, the latter to some degree cancelling the former. (In some transits of the kind, the effect of rotation wholly cancels, or even more than cancels, the effect due to latitude.) The southern station, if taken where, throughout the transit, the observer is on the portion of the disk represented without shading in Fig. 1, will give conspiring effects. The chord of transit $c\,d$ will be shortened, and *Venus* will be hastened on her course. Hence we have for this station two favourable conditions. In all we have three favourable conditions and one unfavourable condition,—so that if the conditions are all equal in value we have a balance of only *two* favourable conditions.

On the other hand, in such a transit as that of 1882 we can theoretically secure four favourable conditions. We have at the northern station the shortened transit-chord $a'\,b'$, and a hastening of *Venus*, or two conspiring conditions. At a southern station we have the lengthened transit-chord $c'\,d'$, and by taking a station which throughout the transit lies on the shaded part of the disk (that is, an Antarctic station passing below the pole during the transit hours), we have *Venus* retarded on her transit-path, or again we have two conspiring conditions. In all then we have *four* favourable conditions, or twice as many as we obtain for the balance of favourable conditions in 1874.

This is theoretically sound. Moreover, it is quite commonly the case that the effects due to rotation are equivalent to those due to latitude, and that therefore the adverse conditions at a station placed as the northern station in 1874, may be regarded as cancelling each other. In the celebrated transit of 1769, for example, the conspiring effects of rotation and latitude were nearly equal. The Astronomer Royal, in his *Popular Astronomy* (published in 1848, be it noticed), justly assigns to rotation 10 minutes out of the observed maximum difference of duration, 22 minutes. It does not seem rash to infer that he had this result in his thoughts (misleading him, in fact) when, after mentioning that the northern stations best placed as respects latitude would probably not be occupied in 1874, he proceeded to remark in 1857 that

the "observable difference" in the earlier transit would "probably not be half of that in 1882" * It is at any rate noteworthy that the investigation then made into the conditions of the transit of 1874 did not claim to be accurate on points of detail,—in fact, the estimated epochs for the beginning and end of the transit were each of them in error by nearly a full hour. So that we may regard the opinion just quoted as based on the general considerations previously indicated, not on any exact investigations of the circumstances of the transit of 1874 in points of detail. The use of the word "probably," in fact, suffices to show this.

But, be this as it may, it is unquestionably the case that those were the last words of the Astronomer Royal on the point in question,—"the observable difference of duration" in 1874 "will probably not be half of that in 1882." When he spoke again about the general subject in 1864, and again in 1865, he confined his remarks altogether to the transit of 1882. In December, 1868, he remarked that Halley's method had been shown to "fail totally" in 1874.

Now let us inquire how far this statement is justified by facts, when details come to be considered.

Let it be noticed that at the northern station the effect of rotation was supposed to cancel, or nearly so, the effect due to difference of latitude. In other words, at the best northern station there would be scarcely any lengthening of the durations. This is a very simple issue, relating to a question of fact, not of opinion. Now in May, 1869 (see our *Monthly Notices* for that month, page 315), I stated that at Nertchinsk, in Siberia, the transit would be lengthened by $15\frac{1}{2}$ minutes. How considerable this lengthening is will be seen at once, when I point out that by imagining the Earth's rotation reversed, so as to conspire with the effect of latitude, we could only obtain as an absolute maximum of difference (with the Sun suitably high both at ingress and egress) the value 21 minutes, so that we may regard $17\frac{3}{4}^{m}$ as the difference due to latitude alone, and capable of being only affected $2\frac{1}{4}^{m}$ either way by rotation; and it never happens in any transit that the absolute maximum of difference is obtainable. But now as to the accuracy of my statement:—Referring to the *Nautical Almanac* for 1874, I find the following statements respecting the mean duration of the transit (for internal contacts) and the duration at Nertchinsk Mines. At p. 434,—

	h	m	s	
Internal Contact at Ingress	14	15	24	G.M.T.
,, Egress	17	57	26	
Consequently the duration of the Transit as seen from the Earth's centre is ..	3	42	2	

* See the *Monthly Notices* for May, 1857, p. 215.

At p. 20 of the Appendix,—

Nertchinsk Mines.		h	m	
Lat. 51° 18′ N.	Int. Contact at Ingress, Dec. 8	22	8·2	Local T.
Long. 119 36 E.	,, Egress, ,, 9	2	5·3	
Consequently the Duration of the Transit as seen at Nertchinsk Mines is		3	57·1	

Or the lengthening at this station is $3^h\ 57^m{\cdot}1 - 3^h\ 42^m\ 2^s = 15^m\ 4^s$.

Now if it be noticed that the place dealt with in the *Nautical Almanac* lies 42′ of latitude south of Nertchinsk itself (at least according to the position given in Phillips's Atlas, Index, p. 22,) and that this difference corresponds to about 25 seconds of time in the duration* (which is, of course, reduced the further south the station is taken) it will be manifest how closely my graphic construction corresponds with the results of the calculations employed in obtaining the corresponding values in the *Nautical Almanac*.

And if further confirmation be required let it be noticed that Russia has in a very practical way adopted the conclusion which I believe I was the first to point out, by selecting Nertchinsk Mines as a station for observing the transit. This station is by no means good for applying Delisle's method; it is inferior to many Russian stations (even) as respects the accelerated ingress, and to Orsk, Omsk, Tobolsk, Perm, Astrakhan, Odessa, and many other stations as respects the retarded egress. It is, moreover, a situation which nothing but an amazing zeal in the cause of science could induce any astronomer to select as an observing station in December,—since it lies close to the northern pole of winter cold (it lies, in fact, on the isocheimenal of 13° F. below zero). But because it is the very best northern station for applying Halley's method, Russia has nobly undertaken to occupy it.† This circumstance, apart from the confirmation it affords to the statements which I published four years ago, is one which ought to influence this country strongly, if it should be demonstrated (as I hope to demonstrate a page or two further on), that there are corresponding southern stations, which this country could occupy, if not less zealous than Russia in the cause of science.

But doubts may still remain whether in stating that "the observable difference in 1874 will probably not be half of that in 1882;" the Astronomer Royal had the difficulty of finding a

* This will clearly be seen by marking in a point 42′ south of Nertchinsk in maps 5 and 6 illustrating my paper in the May number of the *Monthly Notices* for 1869.

† This I infer from the fact that it is included by Mr. Hind among the selected stations, with which alone he undertakes to deal in the Appendix to the *Nautical Almanac* for 1874.

northern station alone in view. Let us, therefore, inquire what is the observable difference, not as between the best northern station and the mean duration, but as between the best northern station and some accessible southern station.

Here, adopting the opinion strongly expressed by the Astronomer Royal (as I shall presently show) that an Antarctic station ought to be occupied if suitable time-differences can thus be secured, we have a very wide choice of places suitable for reconnaissance. But selecting only a station which is known to be accessible, and has been advocated by eminent naval men and geographers (see the *Monthly Notices* for December 1868, and compare the opinions of Captain Richards, Admiral Ommanney, and Commander Davis), namely, Possession Island, near South Victoria Land, let us compare the difference between the durations at this station and Nertchinsk with the maximum observable difference to be obtained in 1882. Still further to favour the transit of 1882, let us in its case take as the Antarctic station Sabrina Land, though the naval authorities above referred to could find nothing to say in its favour. At this latter station, as compared with the best northern stations in North America, there is, according to the Astronomer Royal's correct estimate, a difference of 28^m very nearly. Now at Possession Island the transit of 1874 will be shortened 6^m at the beginning, and $11^m{\cdot}4$ at the end, or $17^m{\cdot}4$ in all. Adding this interval to the lengthening by $15^m{\cdot}1$ at Nertchinsk Mines, we obtain a difference of $32^m{\cdot}5$, that is a *greater* observable difference than in 1882 in the proportion of more than 7 to 6.

But this is far from being all. As a matter of fact, not only is Sabrina Land useless in 1882, because the Sun's elevation at ingress will be only about 4°; but Possession Island (where the difference of elevation will be reduced to 24^m) is also useless, because the Sun's elevation will be only 5° at ingress; and there is no other station where Halley's method can be applied at all advantageously in 1882.

Now in 1874 none of these difficulties present themselves. At Possession Island, which would render available the excellent time-difference of $32\frac{1}{2}$ minutes, the Sun will be 38° high at ingress and 20° high at egress. At Sabrina Land, if this station could be made available, there will be the same time-difference, and solar elevations of 45° and 43° at ingress and egress respectively. At Adelie Land the same time-difference, and the respective solar elevations of 45° and 34°. And lastly, as respects Antarctic stations, at Enderby Land there will be the yet greater time-difference $35\frac{1}{2}$ minutes, and solar elevations 20° and 39° respectively at ingress and egress.

And next, I would invite special attention to the distinct, I may even say the emphatic manner, in which the Astronomer Royal, speaking as the representative of British astronomy, has marked his sense of the duty of this country in the matter of the coming transits. In what I am about to quote he is speaking

throughout of the transit of 1882, but the application of his remarks to the earlier transit (now that the superior advantages of Antarctic stations in 1874 has been demonstrated) cannot fail to be generally recognised.

In 1857, the Astronomer Royal's remarks were thus reported (*Monthly Notices* for May 1857, p. 216): "The southern tract is a part of the Antarctic land discovered by Lieut. Wilkes, of the United States Navy,* included between Sabrina Land and Repulse Bay. The Astronomer Royal is informed by General Sabine that the 6th of December is rather early in the season for a visit to this land, but probably not too early, more especially as firm ice will be quite as good for these observations as dry land. . . . It would be extremely desirable that the country should be reconnoitred some years before the transit." (The whole passage should be studied, but space will not permit me to quote any considerable portion of the passages I refer to here.) Again, in the same Report (p. 221): "The Astronomer Royal argues that the future astronomical public will not be satisfied unless all practical use" (probably a misreport for "practicable use") "is made of the transits of *Venus* in 1874 and 1882, and that for these the determination of some distant longitudes, and a reconnaissance of Wilkes' Land, must be effected within a few years."

The next remarks of the Astronomer Royal on this subject appeared in the *Monthly Notices* for June 1864, pp. 173–177. In this paper, after considering the circumstances of the transit of 1882 (leaving that of 1874 unmentioned), he proceeds to say: "On the whole, I think it very desirable that a reconnaissance should be made of the points under consideration, and that it should not be long deferred. The first locality to be examined is that in 7^h east longitude, between Sabrina Land and Repulse Bay; and the points to be ascertained are, (1) whether the coast is accessible in December 6; (2), whether a latitude of 65° can be reached; (3), whether the Sun can be observed" (under certain conditions which affect the problem unfavourably in 1882, but have no existence in 1874). "Should the answer to the first or third questions be negative, then it would be proper to examine other portions of the south continent, say in longitude not very different from 4^h west, but with no particular restriction except that of gaining the highest possible south latitude."

The next reference to the subject appeared in the *Monthly Notices* for May 1865, pp. 201-203, and the Astronomer Royal's remarks on this occasion are so excellent in themselves, so thoroughly *apropos* of the present geographical position, and apply so forcibly to the circumstances now known to exist in the case of the transit of 1874, that I venture to quote them nearly in full. The paper bears the title, "Letter from the Astronomer

* This Antarctic "land" had however, been sailed over by Ross in 1846–7; and in the later discussion of the subject Sabrina Land was substituted for Wilkes' supposed continent.

Royal to Sir R. I. Murchison, K.C.B., President of the Royal Geographical Society." It runs thus :—

"I have learned, through the public papers, the tenor of late discussions at the Royal Geographical Society in reference to a proposal for an expedition towards the North Pole. I gather from these that the object proposed, as bearing on science, is not so much specific as general ; that there is no single point of very great importance to be obtained, but a number of co-ordinate objects whose aggregate would be valuable. And I conclude that the field is still open for another proposal, which would give opportunity for the determination of various results, corresponding in kind and in importance to those of the proposed Northern Expedition, though in a different locality, and would also give information on a point of great importance to Astronomy, which must be sought within a few years, and which it is desirable to obtain as early as possible.

"In the year 1882, on the 6th of December, a transit of *Venus* over the Sun's disk will occur; the most favourable of all phenomena for solution of the noble problem of determining the Sun's distance from the Earth, provided that proper stations for the observation can be found. (It will be remembered that it was for the same purpose that the most celebrated of all the British scientific expeditions, namely, that of Captain Cook to Otaheite in 1769, was undertaken ; the British part of the enterprise was perfectly successful ; but there have always been doubts of the accuracy of the corresponding observations in Lapland, which render a repetition of the observation very desirable.) In the *Monthly Notices of the Royal Astronomical Society* for 1864, June 10, I have very carefully discussed the circumstances of the coming transit, in reference to the selection of observation-stations. For the Northern stations there will be no difficulty ; they will be on the Atlantic seaboard of North America, or at Bermuda ; all very favourable and very accessible. For the Southern stations the selection is not so easy ; the observation must be made on the Antarctic Continent ; if proper localities can be found there, and if the circumstances of weather, &c. are favourable, the determination will be excellent ; if those favourable circumstances do not hold, no use whatever can be made of the transit."

Then follow certain sentences from the cited *Monthly Notices*, sentences bearing on the selection of Southern stations, and including the passages which I have quoted above. The Astronomer Royal proceeds as follows :—

"The astronomical object of a Southern Expedition is, I trust, sufficiently explained in the sentences which I have quoted. In the event of such an expedition being undertaken, the precise determinations which I have indicated as bearing on the astronomical question must (from the nature of the case) take precedence of all others. But there would be no difficulty in combining with them any other inquiries, of geography, geology, hydrography,

magnetism, meteorology, natural history, or any other subject for which the localities are suitable.

"And I have now to request that you will have the kindness to communicate these remarks to the Royal Geographical Society, and to take the sense of the Society on the question, whether it is not desirable, if other scientific bodies should co-operate, that a representation be made by the Royal Geographical Society to Her Majesty's Government on the advantage of making such a reconnaissance of the Southern Continent as I have proposed; primarily in the interest of Astronomy (referring to my official responsibility for the importance of the examination at this special time); but conjointly with that, in the interests, perhaps ultimately more important, of geography and other sciences usually promoted by the Royal Geographical Society."

I need scarcely remind my readers of the paper read before the Society by the Astronomer Royal in December 1868. Nevertheless, it is necessary, first, to point out that at that late epoch the error respecting the transit of 1874 still remained uncorrected, and that the Astronomer Royal then (see p. 33 of the *Monthly Notices* for December 1868) repeated that "the method by observation of the interval in time between ingress and egress at each of the stations at least, on nearly opposite parts of the earth, fails totally for the transit of 1874." At that time also, notwithstanding the relatively unfavourable circumstances for applying this method (Halley's) to the transit of 1882, and the very favourable conditions under which Delisle's method can be applied in 1882, he urged that only three stations should be occupied for Delisle's method in that year, the instruments of the five 1874 Expeditions, "thus set free from two stations," being required at an observing station on the Southern Continent. He had now so far changed his mind as to the method of dealing with Antarctic difficulties, as to speak in the following terms:— "The choice of station being made," he said, "I would not recommend any reconnaissance, but I would propose that an expedition should go direct to the selected point in good time for the observation of the phenomenon. The season is early for South Polar expeditions, and any difficulties produced by ice would probably diminish every day. A station being gained, all that is necessary in the way of subsidiary observation is, a few days' observation to give clock-rate, then the clock times of the two phenomena will furnish all that is required. The first action to be undertaken by the Government," he proceeds (and I invite special attention to the point), "is to procure the stock of instruments, and this ought to be done without delay. An observing plant like that," (described in the earlier part of the same paper), "is not to be obtained in haste, and the proposed expedition might be entirely crippled by a small negligence on this point. The equipment of ships and the selection of officers would probably require much less time."

It will be noticed that if such a plan as this could be followed

out in 1874, the necessity of wintering in Possession Island would be avoided. However, it appeared to the naval authorities who followed the Astronomer Royal in addressing the meeting, that the more certain course for achieving the desired result would consist in the preparation of an expedition to winter in Possession Island. I quote the following passages as bearing specially on the feasibility of such an expedition :—

Admiral (then Captain) Richards, Hydrographer to the Admiralty, said, "My own opinion, looking to the uncertainty of finding a wintering station for a ship, is that landing a party on Possession Island," or one of the islands farther south, "would be the most feasible course, and there would be little doubt of the facility of reaching one or other of these islands with a suitable steam-vessel, making Tasmania or New Zealand the base of operations. Doubtless a year passed in this region would be most profitably employed in adding to our knowledge of magnetism, and various other branches of physical science."

Admiral Ommanney said, *inter alia*, "I fully concur in all that has fallen from the Hydrographer to the Navy, and hope ere long to hear that operations are making for sending out to explore the Antarctic Seas."

Commander J. A. Davis, who had accompanied Sir James Ross in that most gallant expedition during which Victoria Land was discovered, and who had himself landed at Possession Island, said that "he believed there would be no difficulty whatever in again effecting a landing in the same place." "With regard to the period of the season at which the transit took place, it was to be remembered that the 6th of December was so early that no ships had ever reached the Antarctic Circle by that date ; and as it would be necessary to arrange the instruments, &c. preparatory to the observation, he might say that the ships ought to be on the spot at least a month before. This would be the 6th of November, a date altogether out of the question ; and as the ships could not winter in the South, the party would necessarily have to land the year before ; but with good tents he had no doubt they could pass the winter very comfortably" (this, of course, and what follows, will not be taken strictly *au pied de la lettre*) ; "they would have a pleasant prospect before them, and plenty of penguins to live on. In comparison with Kerguelen Island and the Crozets," he proceeded, "the chances of observing the transit— meteorologically speaking — would be greatly in favour of South Victoria."

Captain Toynbee also expressed an opinion strongly adverse to the meteorological chances at Prince Edward's Islands, the Crozet's, and Kerguelen Land, since their neighbourhood is, he said, "so far as my experience goes, subject to a great deal of thick weather."

It remains only to mention that the chart which illustrates this paper, besides being useful in showing the path of *Venus*' centre across the Sun's disk, as seen from the stations named, and

indicating the corresponding path for any station whatever, affords an independent proof of that which, however, has already been abundantly demonstrated—the fact, namely, that Halley's method is most advantageously applicable in 1874. The chart requires little explanation. The simplest geometrical considerations will show that, imagining a long line to extend through *Venus*' centre at any moment during transit to the Earth on one side, and the Sun on the other; then if the end towards the Earth be supposed to be carried swiftly along the outlines of the terrestrial continents, and over the meridians and parallels, the end towards the Sun would trace out such projections as are shown in the chart. Moreover, it is manifest that from whatever point on the Earth such a line extends, the point in which the line meets the Sun is that on which *Venus*' centre is at the instant projected. Accordingly, we have only to determine the aspect of the Earth's disk as seen from the Sun at any instant, and the position of *Venus*' centre, on the Sun's disk as seen from the Earth's centre at that instant, to have at once the means, by constructing such an inverted projection as is seen in the successive pictures of the chart, of determining the apparent position of *Venus*' centre as seen at that instant from any point of the Earth's sunlit hemisphere.

The chart itself shows clearly the relation between the strip of the Sun's disk (divided into three portions in the chart) and the outline of that disk. Moreover, the circles marked $\frac{1}{2}\odot + \frac{1}{2}\,♀$, and $\frac{1}{2}\odot - \frac{1}{2}\,♀$ indicate by their intersection with the various transit clouds, where external contact and internal contact respectively take place; for, manifestly, when *Venus*' centre, as seen from any station, is on the circle marked $\frac{1}{2}\odot + \frac{1}{2}\,♀$ (that is, is at a distance from the Sun's centre, equal to the sum of his radius and *Venus*'s), *Venus* must be at the moment, and as seen from that station, in external contact, and similarly she must be in internal contact when her centre, as seen from any station, is on the circle marked $\frac{1}{2}\odot - \frac{1}{2}\,♀$.

But fig. 3 serves to show more clearly how the illustrative chart is to be interpreted. It shows the northern half of the Sun's disk, and indicates the relative dimensions of the disk of *Venus* and the Sun, as well as the maximum parallactic displacement of *Venus*.

I apprehend that it has been demonstrated that (i) the Astronomer Royal's first and only discussion of the suitability of Halley's method in 1874 was based on insufficient evidence, was in itself incomplete, and led him to an erroneous opinion; (ii) that not only is the method more advantageously applicable in 1874 than in 1882, as regards time-difference, but that the objection of low solar altitude at a critical phase in 1882 has no existence in 1874; (iii) that the Astronomer Royal himself warmly advocated the equipment of Antarctic expeditions for viewing the transit of 1882 by Halley's method, notwithstanding the known difficulties; and (iv) that the best naval authorities on this

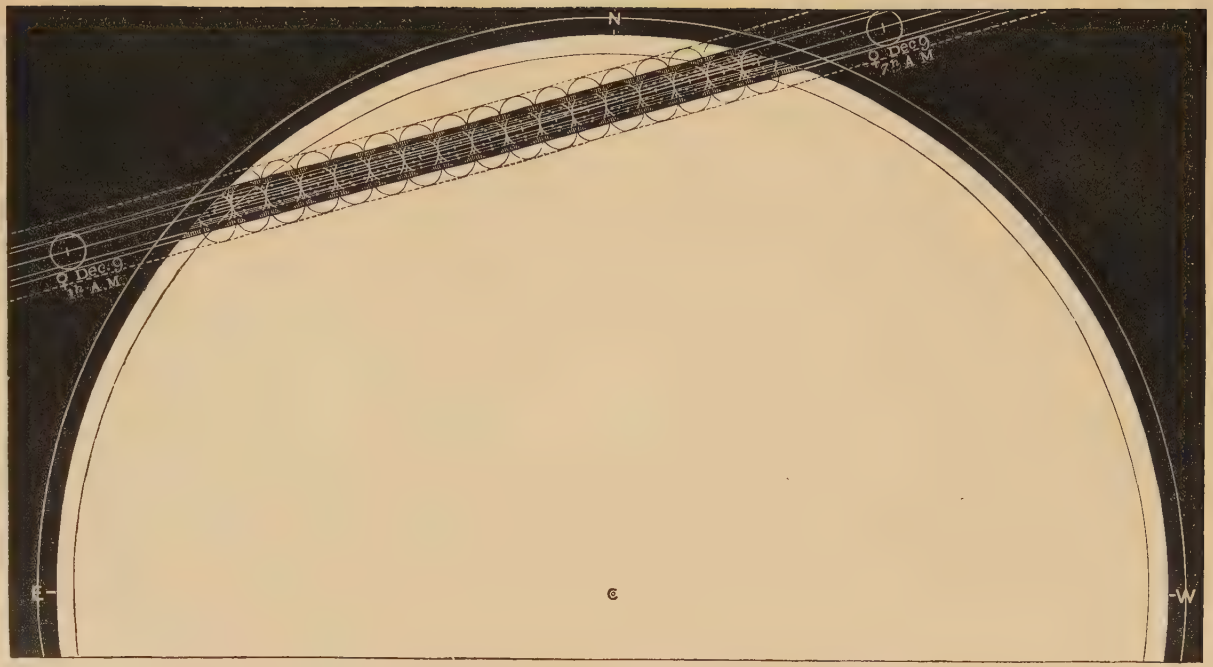


FIG. 3.—Illustrating the Transit of *Venus* across the Sun's Disk, on December 9, 1874.

special subject concur in regarding Antarctic expeditions for viewing a transit early in December as altogether practicable.

The conclusion directly deducible from these results cannot be mistaken. England's duty is more than manifest ; it has been to all intents and purposes admitted by her astronomical and nautical official representatives. And I cannot but express my conviction that it will be little less than a national calamity, as assuredly it will be scientifically most regrettable if any considerations, either of convenience or of personal dignity on the one hand, or of false courtesy on the other, should lead to the loss of opportunities which will not be again available for many years to come.

Note on the direct Determination of the Parallactic Displacement of Venus during her Transit. By Richard A. Proctor, B.A. Cambridge.

Very early during my examination of the subject of the approaching transits, I was led to adopt and state the opinion that the parallactic displacement of *Venus*, and thence the Sun's parallax, might, in the present state of instrumental astronomy, be determined at least as accurately by direct measurement of *Venus's* position at successive epochs of her transit as by either Delisle's or Halley's method. It appears impossible to eliminate the error resulting from the clinging of *Venus* to the Sun's limb, after ingress and before egress ; and although several contrivances have been suggested for reducing this error, it is doubtful whether any of them will prove successful. It remains to be shown, moreover, whether photography can be successfully applied to determine the parallactic displacement of *Venus*.

I find that the German astronomers have for some time recognised the advantages which would probably result from such processes of measurement as I have mentioned; and their selection of Tchefoo, where the whole transit will be observable, indicates in a marked manner their preference for the direct method, since Tchefoo is not an exceptionally advantageous station for observing the accelerated ingress, and still less for observing the retarded egress. It is also inferior to other northern stations for applying Halley's method ; and indeed German astronomers have definitely indicated their preference for the direct method.

The American astronomers have also adopted a favourable opinion as to the direct method.

It appears to me that if English astronomers are to base their methods of procedure on foreign opinions (a growing fashion which I myself am far from urging as desirable) attention might not disadvantageously be directed to the considerations resulting from the above-mentioned opinions of German and American

astronomers. It is easy to perceive what these considerations are.

In the first place, it is manifest that, *cæteris paribus*, these stations will be most advantageous which show the whole transit under the most favourable conditions ; and in comparing these stations, we should regard that station as the better which shows the greater proportion of the transit favourably. So that, so far as this method of observation is concerned, our selected stations at Woahoo and Alexandria would be altogether inferior to such a station as Tchefoo, since at Woahoo the Sun sets before half the transit has taken place, and at Alexandria more than half the transit is already over when the Sun rises.

It is probable, however, that few would be disposed to sacrifice such a station as Woahoo, where Delisle's method is applicable under conditions exceptionally advantageous. But it may be worth while to inquire whether so much can be said in favour of Alexandria : in fact, it appears tome that but one answer can be made to this question, so far as it relates to the action called for on England's part.

To begin with, it seems not wholly unreasonable to expect that either Italy or Greece should occupy the only really advantageous Mediterranean station. Moreover, France is to occupy Suez, and the circumstances of the transit at Suez and Alexandria will be very nearly identical, as can be seen at once from the chart which illustrates the present number of the *Notices*.

But it is when we compare the circumstances of the transit at Alexandria with those which will be presented at North Indian stations, such as Peshawur, Delhi, and so on, that we find most occasion to regret the unfortunate accident by which these North Indian stations came to be so long and so completely overlooked.

As respects the application of Delisle's method, the advantage of Peshawur is sufficiently marked. The retardation is indeed but a third of a minute greater at Peshawur than at Alexandria. But the Sun is unfortunately very low at Alexandria —only 14° above the horizon —when egress occurs, whereas at Peshawur the Sun will be more than 31° above the horizon.

The main point, however,—and I conceive it to be an extremely important point,—is that at Peshawur, at Delhi, and at many stations over the region between these places, the whole transit will be visible, and therefore processes of direct measurement can be most effectively applied. When we add to these considerations the circumstance that it is much more manifestly the duty of Great Britain to occupy this advantageous region in her own territory than to occupy Alexandria, but one opinion can be formed.

When I dwelt on these considerations, some nine months since, in a letter addressed to the Astronomer Royal, he pointed out that it was easier (and more advantageous to geography) to determine the latitude of Alexandria than that of Peshawur, and moreover that the position of Peshawur, near the Khyber Pass,

was altogether unfavourable. Knowing something, however, of the zeal with which the survey of India is being prosecuted, and of the importance of several exact determinations of the longitudes of Indian stations, I am not altogether convinced that the determination of the longitude of Peshawur would present insurmountable difficulties or be utterly useless in a geographical sense. I observe also that the Indian railway system is to be extended to Peshawur, and therefore I venture to surmise that the place is not altogether inaccessible.

I note, in conclusion, that stations useful for Halley's method are (in the present instance) always useful, and sometimes among the very best stations for Delisle's method, while they are manifestly the most advantageous stations for the direct method. The inference is too obvious to need enforcing.

Note on the Nautical Almanac Values of the Semidiameters of the Sun and Venus used in the calculations of the Transit of Venus. By Edwin Dunkin, Esq.

In a Report prepared by Mr. G. W. Hill, entitled, "Charts and Tables for facilitating predictions of the several phases of the Transit of *Venus* in December, 1874," forming Part II. of "Papers relating to the Transit of *Venus* in 1874," published by authority of the United States Government, the following remarks on the value of the semidiameter of the Sun, used in the computations of the *Nautical Almanac*, occur on page 8, "In the British *Nautical Almanac* the value 961″·82 is used, and is the same as that given for the reduction of meridian-observations of the Sun. Le Verrier states (*Annales*, vol. vi. p. 40) that the value deduced from the previous transits of *Venus* is 958″·424. Hence it is probable that predictions from the elements of the British *Nautical Almanac* will be found to be considerably in error from this cause."

With reference to these remarks, and to prevent any misunderstanding on this important point, it is but proper to state that the value 961″·82, printed in the Preface to the *Nautical Almanac*, has not been used in the computations relating to the transit of *Venus*. This has been evident to every one who has had occasion to examine the elements inserted on page 434 of the *Nautical Almanac* for 1874. A very slight comparison of the tabular semidiameters of the Sun and *Venus*, contained in these elements, with the corresponding semidiameters in the solar and planetary sections, will show at once that *corrected* values have been adopted in the calculation of the phases of the transit as given in the *Nautical Almanac*. The amount of the correction is also clearly evident, by noting that the true semidiameter inserted on

page 434 is 0″·6 less than that for December 9 on page 223. A comparison of the semidiameter of *Venus*, given on pages 313 and 434, will show the amount of correction applied to the planet's semidiameter.

Since my attention has been drawn to the paragraph quoted from Mr. Hill's report, I have been informed by Mr. Hind that the adopted corrections have been deduced from the meridional observations of the Sun and *Venus* made at Greenwich. The correction applied to the *Nautical Almanac* semidiameter of the Sun is therefore — 0″·53, which is the same as that given in the Introduction to the *Greenwich Observations*, as the mean result of numerous comparisons of the observed and tabular diameters. Mr. Hind has availed himself of Mr. Stone's value of the semidiameter of *Venus*, as the best possible modern determination, resulting as it does from a considerable number of vertical measurements of the diameter observed with the transit-circle at the Royal Observatory. (*Monthly Notices*, vol. xxv. pp. 57–59.)

Kidbrooke, Blackheath,
March 6, 1873.

On the Markings on Venus. By J. M. Wilson, Esq.

As some doubt exists whether markings are ever really seen on *Venus*, it may be worth while to call attention to the present appearance of the planet. There certainly *seems* to be the same bright region round the north cusp, separated by a darker region from the rest of the surface, that was frequently observed here in 1871. But it would be well that it should be examined by others.

The instrument used here is Alvan Clark's 8¼-inch, powers various.

Temple Observatory, Rugby,
Feb. 13.

Note by Mr. Denison on his Paper on the Barometric Error of Clocks.

Since the printing of my paper of last January I have found that Dr. Robinson's barometric compensation was the same in principle as mine, though I inferred the contrary from the way he spoke of it, and he gave no reference to his own description, which I find in the 5th volume of the *Memoirs*. He used two barometers entirely above the bob, while I think one sufficient, as a slight want of symmetry in the plane of vibration cannot affect the pendulum sensibly, if at all. And with an iron jar, which all the best pendulums now have, the barometer cannot be

above it, for the reason which I gave before. These, however, are mere matters of detail, and I am glad to see that such a compensation is "perfectly successful."

It may be as well to mention, that "Baily's result for a detached one, viz. 0·41 sec.," mentioned by Dr. Robinson, does not mean a "detached escapement;" but what is more generally called a *free* pendulum, set vibrating without a clock, and observed as long as it will swing a considerable arc. The number of values of the barometric error which have now been mentioned proves the necessity of ascertaining it, by trial of the individual clock, before calculating the correction for it.

I had not seen the Westminster pendulum for some years until the other day, when I was asked to explain the clock to the members of the Horological Institute, and I find the arc is even larger than I stated from memory, and, as near as I could see, is $2^\circ\ 45'$; the very arc at which Baily calculated that the circular error ought to compensate the barometric (*Phil. Trans.* of 1832). This confirms the conclusion arrived at before from the rate of the clock during last year.

I have also been told by a very able clockmaker, Mr. Brock, of George Street, Portman Square, that he has often found so many clocks affected in the same direction at the same time, with no apparent cause, that he had come to the conclusion that they are affected by the electrical state of the atmosphere, as they certainly are sometimes by thunderstorms. That point is worth attending to by those who have the means of doing so, now that the electricity of the atmosphere is regularly observed.

On Mr. Denison's Compensation for the Barometric Error of Clocks. By the Rev. T. R. Robinson, D.D.

In Mr. Denison's paper on this subject there are two points which require a few words from me: one, his statement, "that it seems that it" (the compensation) "had been done in some way;" the other, "that I had found difficulty in calculating the adjustments for it."

If Mr. Denison will look at page xix. of the Introduction to the *Armagh Catalogue*, he will find a description of my compensation, and will see that his own differs from it only by the complex and inconvenient arrangement of his tubes.

There is no difficulty in calculating the diameter and position of the barometers which shall compensate a given pendulum; but there is much in making the working of that compensation perfect. This can only be done by applying the method of of minimum squares to a large number of rate observations, extending over periods which shall include the extreme annual range of temperature. Three unknown quantities must be determined;

and the process must be repeated for every change of the barometer tube's position which involves also changes of rate and heat-compensation.

All this work is required with Mr. Denison's compensation precisely as with mine. It was seven years before I was satisfied with my work ; and it was this experience which induced me to recommend, in preference to my own invention, the placing the pendulum in an atmosphere of hydrogen.

On the Barometric Error of Clocks. By R. Webster, Esq.

The paper of Mr. Carrington in the *Monthly Notice* for November, "On the rate of a Clock going in a Partial Vacuum," and the subsequent papers by Dr. Robinson and Mr. Denison in the January number, have led me to investigate the same subject.

Since May 1872, I have had a telegraphic wire direct from the Royal Observatory, conveying hourly signals of Greenwich mean-time, by striking on a bell. In the same room with this telegraphic apparatus, I have several astronomical clocks with gravity and dead-beat escapements. I have selected the best of these, the maximum deviation of which from Greenwich mean-time, since November 11th, 1872, to the present time, has not exceeded two seconds on each side of 0, and this I principally attribute to the barometric error. It has a dead-beat escapement, with jewelled pallets, jewelled escape holes, pinions of sixteen, mercurial pendulum with iron jar, and the arc of vibration is 1° 58′ on each side of 0.

On February 18th, a dense atmospheric wave passed over London, and the barometer indicated 30·76. A daily fall occurred until the 26th, when it marked 29·00, giving a range of 1·76 inches in eight days.

The following is the rate of the clock, the height of the barometer and the thermometer :

	Sec.	Bar.	Ther.
Feb. 17	− 0·1	30·22	46
18	− 0·3	30·76	47
25	0·0	29·68	44
26	+ 0·2	29·00	49

By taking the difference of rate on the 18th and 26th, the days of maximum and minimum of barometer we have

$$\frac{0\cdot50}{1\cdot76} = 0\cdot28$$

Comparing the 17th and 18th,

$$\frac{0\cdot20}{0\cdot54} = 0\cdot37,$$

and comparing the 25th and 26th,

$$\frac{0\cdot20}{0\cdot68} = 0\cdot30.$$

The mean of these observations is 0·32.

Comparisons with the recorded rate of this clock and unusual variations of the barometer corroborate this result.

It may therefore be fairly assumed that for this clock the co-efficient of barometric retardation between 29·00 and 30·76 is 0·32 seconds per inch per diem.

New Use of the Altazimuth Diagram.

By the Rev. A. Freeman, M.A.

J. M. Wilson, Esq., of Rugby School, has suggested to me that the diagram, of which a sketch was given in the *Monthly Notices* for November 1872, at p. 23, may be used for quite a different purpose from mine ; namely, for refraction corrections when observing out of the meridian with an equatoreal.

This is indeed easily understood. Refraction displaces a star towards the zenith along the line which represents the vertical circle passing through the star's true place as assigned with respect to polar distance and hour-angle. The effect on these two co-ordinates may be estimated, approximately, by a glance at the diagram, with the aid of the annexed short list of Bessel's mean refractions, taken from Loomis' *Astronomy*, Table VIII.

Mean Refractions corresponding to Zenith Distances.

Z.D.	M.R.	Z.D.	M.R.	Z.D.	M.R.
°	′ ″	°	′ ″	°	′ ″
10	0 10	70	2 37	84	8 23
20	0 21	72	2 56	85	9 47
30	0 33	74	3 19	86	11 39
40	0 48	76	3 47	87	14 15
45	0 58	78	4 25	88	18 9
50	1 6	80	5 16	88½	20 51
55	1 22	81	5 49	89	24 25
60	1 40	82	6 29	89½	29 3
65	2 3	83	7 20	90	34 54

St John's College, Cambridge,
Feb. 1, 1873.

Results of Observations of Shooting Stars, made in the Mediterranean in the years 1869, 1870, *and* 1871. By Captain G. L. Tupman, Royal Marine Artillery.

In the *Monthly Notices*, vol. xxxiii. page 345, is a paper, by Mr. R. P. Greg, on the Radiant Points and Durations of Shooting Star Showers, which is undoubtedly the most complete and the most accurate that has ever been published. Dr. Heis of Munster, Dr. Julius Schmidt of Athens, Professors Schiaparelli and Zezioli, and others have also, from time to time, published catalogues of radiant points more or less rich ; but, with the exception of those of Dr. Schmidt and Prof. Neumayer (in Australia), they contain chiefly the positions of radiants in the northern hemisphere, determined from observations made before midnight.

During three years lately spent cruising in the Mediterranean, I, at the suggestion of Dr. Schmidt, devoted a considerable time to the determination of such radiants as occur between midnight and sunrise, with large north polar distances. The method of observation generally adopted was to keep the equator to the southward in the centre of the sphere of vision, to count the number of shooting stars that appeared during the whole period of observation, to delineate on star-charts as many as possible of the tracks, and to enter in a note-book the time by chronometer, colour, estimated duration, magnitude, &c. In this manner 3800 meteors were observed on 154 mornings free from moon or cloud, the average duration of an observation being a little over two hours. 2000 tracks were registered, and from these the radiant positions were deduced, the other 1800 telling in the horary numbers and richness of streams.

In order to carry out these observations, I found it necessary to construct star-charts on purpose, of fine London board, on which the stars were represented with common ink. No printed charts would bear the continual erasure of the pencil-marks. Since only the equatorial regions were required, I chose the cylindrical projection on a scale of 10° of R.A. to the inch, and inserted all the stars in the Astronomical Society's Catalogue down to the sixth magnitude inclusive, the lines of R.A. and declination being drawn in a colour that was invisible by ordinary lamp-light.

For observations of the great August stream, I used a copy of Plate XI. of the British Association Gnomonic Atlas for 1868. The epoch of the Perseus Radiants is therefore 1850, of the others 1830.

From the necessity of clearing off the tracks every second or third day (and sometimes every day), doubtless many poor streams escaped detection. The complete reduction of such a mass of observations would require more time than I can devote to it, while I think very little would be found to repay the labour. Most of the streams would be a little enriched, a few new ones discovered,

and perhaps a few of the old discarded. Of the 102 radiants whose positions are given in the Catalogue, 58 are identical, and 21 others are fairly in accordance with those of other observers; and as the remaining 23 were found in a precisely similar manner, and, on the average, by the same number of close meteors, there is little doubt of nearly the whole of them being true radiants.

The following table shows the disposition of radiants in zones of north polar distance, according to the largest published lists. The first is Mr. Greg's list already alluded to, which contains the British and Italian lists combined; the second is the list of Dr. Schmidt from the *Astronomische Nachrichten*, No. 1756,* (the result of 3000 observations); the third is that of Prof. E. Heis, as published in the B.A. atlas for 1868, and the fourth is my own.

Distribution of the Radiants in Zones of N.P.D.

N.P.D.	Greg.	Schmidt.	Heis.	Tupman.
° to °				
0 to 30	25	10	33	6
30 „ 60	67	21	36	23
60 „ 90	34	39	15	42
90 „ 120	7	28	0	31

The results of special observations of the great August stream are given in an appendix, together with those of other observers, and the whole are represented on the accompanying chart. Mr. Greg describes the radiant as an elongated area extending from *Perseus* to *Cassiopeia*, the co-ordinates being $\alpha = 50°$ to $25°$, $\delta = + 50°$ to $65°$, with a special centre at $44° + 56°$. Professor Serpieri gives $\alpha = 50°$ to $30°$, $\delta = + 49°$ to $64°$, and all observers agree in a well-defined centre identical with Mr. Greg's.

The branch, or secondary streams do not appear to separate from the principal according to any law.

I cannot close these remarks without expressing my great obligations to Mr. Greg for carefully condensing my original catalogue into nearly its present form, and for having pointed out most of the agreements with his own and Dr. Schmidt's.

The columns of the catalogue require no other explanation than that the Zenith Horary Number, where given, has been obtained by multiplying the number of meteors counted per hour by the secant of the zenith distance of the radiant. It represents the number that would probably have been counted by a single observer with the radiant in his zenith. The numbers in the general table which follows are, of course, not similarly affected. It may be mentioned that the *sphere of vision* for small meteors is about 45°, and for large about 90° in diameter.

* As abridged by Mr. Greg in an MS. copy kindly lent to me.

Catalogue of Observed Radiant-Points and Comparison of the Positions with those of other Observers.

No.	Date of Observation.	R.A. and Declination for 1830.	Zenith Horary Number.	(D.), Prof. Denza (Memorie, etc.) (H.), Dr. Heis. (N.), Prof. Neumayer. (S.), Dr. Schmidt. (A.S.H.), Prof. Herschel.	Mr. Greg's Numbers and Dr Heis' Letters.	Dr. Schmidt (Athens) Astr. Nach. No. 1756.	
		° °					°
1	1870, Jan. 2-7	229 + 51	6½	Masters, 1863, Jan. 2 (A.M.) 238° + 46½°	4, K_{1-8}	..	
2	1869, Dec. 23-31	160 + 3	..	..	22		
	1870, Jan. 8-10	165 + 4	..	..	S_1		
	Jan. 11	173 + 9	..	..	S_2		
	Feb. 3-10	175 + 16	..	(N), Feb. 174° + 16°	..	March	162 +
3	1870, Jan. 4	142 + 5	..	..	..		
	Jan. 11	149 + 5	..	..	? 112	Dec.	145 +
4	1870, Jan. 4-31	180 + 35	..	Not accurate	8, ? M_2		
	Feb. 3-10	180 + 35	..	,,			
5	1870, Jan. 5	185 + 15	..	..	22		
	Jan. 11	173 + 9	..	..	$S_{1,2}$	March	162 +
	Feb. 3-10	175 + 16	..	(N.) March, 174° + 16°			
6	1870, Jan. 5, 11	210 − 6	..	..			
	1869, Feb. 13	202 − 9	..	..			
	1870, March 2-3	200 − 10	..	..			
7	1870, Jan. 5	145 − 25	..	See 18			
8	1870, Jan. 8-11	200 + 2	..	See 21	? 31		
	1869, Feb. 13	205 + 4	..				
9	1870, Feb. 3-10	210 + 36	..	S. and Z. 209° + 25°	? 9, 15		
10	1870, Feb. 3-10	237 + 20	..				
	1869, Feb. 13	237 + 13	..	Accurate. ? 19, 27	? 15		
11	1870, Feb. 3-10	198 − 22	..	See 17			
	1869, March 19	202 − 28	..				
12	1870, Feb. 3-10	219 − 23	..				
	1869, Feb. 17	218 − 13	..				
13	1870, Feb. 8-10	180 − 20	..				
14	1870, Feb. 10	290 − 12	..	Not accurate	41		
15	1869, Feb. 13	230 − 7	..	Suspected	40		
16	1869, Feb. 13	260 ± 0	..	Suspected, see 21			
17	1869, Feb. 22	217 − 41	..	Suspected			
	Mar. 11	205 − 33	..	,,			
	Mar. 19	202 − 28	..	,, ? 11			
18	1870, Mar. 1	142 − 25	..	Suspected. ? 7			
19	1870, Mar. 2-3	247 + 18	..	See 10, 27			
	1869, Mar. 7	244 + 15	11		? 51		
20	1870, Mar. 2-3	260 + 19	..	Lynn 273°·0 + 25°·5	? 38		
21	1869, Mar. 14-15	266 + 6	..				

Date of Observation.	R.A. and Declination for 1830.	Zenith Horary Number.	(D.), Prof. Denza (Memorie, etc.) (H.), Dr. Heis. (N.), Prof. Neumayer. (S.), Dr. Schmidt. (A.S.H.), Prof. Herschel.	Mr. Greg's Numbers and Dr. Heis' Letters.	Dr. Schmidt (Athens), Astr. Nach. No. 1756.	
1870, Mar. 2-3	209°+18°	..	..	S. 5—6		
1870, Mar. 2-3	236—13	..				
1869, Mar. 7	233—18	9	..	? 40		
1870, Mar. 2-3	193+ 1	..	(H.) Apr. 194°+5°	31		
1869, Mar. 19	194+10	..		? S_2		
1870, Mar. 2-3	240+39	..				
1870, Mar. 7	186— 9	..				
1869, Mar. 7	241+ 8	..	Suspected	30		
1870, Mar. 7	246± 0	..	See 10, 19, 31			
1870, Mar. 7	270—22	..	After 3 A.M. only			
Apr. 25	273—30	..	Suspected.			
1870, Apr. 27	286—27	..				
1870, Apr. 27	256— 2	..	Accurate.	? 30		
1870, Apr. 27-28	230+ 5	..	B.A. atlas confirmed.	? 40		
1869, Apr. 29	329— 2	..	Fairly accurate.			
1870, Apr. 30	325— 3	15				
May 2-3	325—2½	21	Fine shower, May 2			
1870, May 2	285+12	..	(H.), June 1-30, 292°+15°	? 56 ? W	July 5-25	278°+13°
1870, May 2	298+ 5	..	..	? 57, 63	June	313+12
June 28	305+ 9	..	(N.), 305°+5°	? W	July 5-28	314+10
1870, June 28	293—11	..	(S.) confirmed	..	June	293—11
June 29	284—16	..	(N), June, July 263°—15°	..	July 18-31	287—21
July 1	287—13	..	..	..	June	282— 3
1870, June 28	305— 7	..	(N.) confirmed (305°—6°)	..	July 5-25	301— 5
July 22	306— 8	..	(S.) confirmed	..	Aug. 1-31	306— 8
1870, June 28	338+13	..	B.A. atlas confirmed	67	June	335+10
July 1-6	337+ 1	..	..	..	July 5-28	335+ 7
					July 20-31	334+ 1
1870, June 29-30	280+29	..	B.A. atlas confirmed	56	June	284+38
1870, July 1	235± 0	..				
1870, July 1	N. Pole	..				
1870, July 8	316+22	..	..	T_1, ? 80	June	319+32
					July 5-30	317+32
1870, July 27	340—14	..	Accurate,		July 20-31	333—14
1870, July 28	340—19	..	(N.) Aug.-Sept. 341°— 6° 337°—10° Weiss, 1869, Aug. 11-13, 338½— 5	130	July 20-31	340— 8
					Aug. 1-31	344—11

No.	Date of Observation.	R.A. and Declination for 1830.	Zenith Hourly Number.	(D.), Prof. Denza (Memoire. etc.) (H.), Dr. Heis. (N.), Prof. Neumayer. (S.), Dr. Schmidt. (A.S.H.), Prof. Herschel.	Mr Greg's Numbers and Dr. Heis' Letters.	Dr Schmidt (Athens), Astr. Nach. No. 1756.	
		° °					°
44	1870, July 21	320— 4	..	..		July 25-31	324—
	July 28	326—13	..	..	130	Aug. 1-31	328—
						Aug. 3-31	331—
45	1870, July 27	7 + 32	7½	(D.), 1869, Aug. 10, 0° + 28°	84	Aug. 1-31	11 +
	Aug. 23	0 + 33	..	(H.) Aug. 1-31, 11° + 30°		Sept.	3 +
46	July 27—Aug. 28	Perseus	..	See Appendix.	A 9-12		
47	1870, July 29	45°—21°	..	Just before daylight.		Aug.	55 —
48	1869, Aug. 4	41 + 34	..	Schiaparelli, Aug. 10, 41° + 34°.			
	1870, Aug. 22	39 + 28	..	Weiss, 1869.	82	Aug. 3-12	54 + 2
	Aug. 28	39 + 28	..	Aug. 11, 46½ + 23°½. Aug. 12, 41½ + 24. (D.) 1869, Aug. 10, 48 + 19.	132		
49	1869, Aug. 6	0 + 17½	..	(H) Sept. 1° + 11°.	88	July	0 + 1
	Aug. 18	7 + 13	..	Oct. 3 + 11.	? T_3	Aug. 1-31	13 +
						Sept. 3-10	17 +
50	1871, Aug. 13	310 + 58	5½	(H.) Aug. 15-31, 306° + 59°. (D.) Aug. 10-11, 304° + 41°.	? 68	Aug. 4-9	304 + 6
					B_6	Sept.	309 + 6
51	1870, Aug. 22	340 + 33	..	(D.) 1869, Aug. 11, 350° + 24°.	76	July	345 + 2
						Aug. 1-15	338 + 2
						Oct.	345 + 2
52	1870, Aug. 22	300 + 85	..	(D.) 1869, Aug. 10, 239° + 75°. Aug. 11, 298° + 83°.	75. H_{13} 15		
53	1870, Aug. 22	82 + 67	..	Suspected. Weiss, 1869, Aug. 11, 77° + 54°.	? 97		
54	1870, Aug. 23	334 + 48	5	Accurate. B. A. Atlas, Aug. 7.—Sept. 30, 335° + 52°. Weiss, 1869, Aug. 12-13, 345° + 50°	96	Aug. 7-31	347 + 2
					? B_2	Sept.	354 + 4
						Oct. (end)	340 + 2
55	1870, Aug 23.	295 + 28	3	Accurate.			
	,,	302 + 21	–	Sub-radiant.	? T_1	? Aug.	311 + 3
56	1870, Aug. 23	297 + 2	..	Weiss, 1869, Aug. 11-13, 298° + 8½°. (D.) 1869, Aug. 10-11, 296° + 6°.	? 63		
57	1871, Aug. 20-25	358 + 6	..		T_3. ? 88	July	7 +
						Aug.	3 +
58	1871, Aug. 20-25	280 + 58	..	B. A. Atlas, July-Aug. 280° + 55°. (D.) 1869, Aug. 11, 277° + 54°.	? 71, B_7	Sept.	290 + 5
59	1871, Aug. 20-25	264 + 64	..	..	? 83	? Aug. 4-11	252 + 5
60	1871, Aug. 20-25	110 + 32	..	Suspected.			
61	1871, Aug. 20-25	5 + 49	..	Declination accurate. ?38.	78 ? 84	Oct. 22-28	5 + 5
62	1871, Aug. 20-25	23 + 36	..	Accurate. Denza, 1868-9. Aug. 8-13, 15° + 34°.	98 ? P_1	? Aug.	23 + 2

o.	Date of Observation.	R.A. and Declination for 1830.	Zenith Horary Number.	(D.), Prof. Denza (Memoire. etc.) (H.), Dr. Heis. (N.), Prof. Neumayer. (S.), Dr. Schmidt. (A.S.H.), Prof. Herschel.	Mr. Greg's Numbers and Dr. Heis' Letters.	Dr. Schmidt (Athens), Astr. Nach. No. 1756.
		° °				
3	1871, Aug. 20-25	330 +12	..	..	67. ? T_2	Aug. 338° +17 Aug. 325 + 1
4	1870 Aug. 29	70 +31	..	See 72, 83.	? 94, A_{16}	Sept. 70 +23
	1871, Sept. 7-15	66 +40	..			Oct. 10-27 71 +31
5	1871, Aug. 20-25	53 + 1	..	(S.) Confirmed.	..	Aug. 53 + 1 Aug. 3-12 55 + 7 Oct. 50 + 2
6	1870, Aug. 29	75 +45	..	(D.) 1869, Aug. 10, 61°+43°.	? 100	
7	1870, Aug. 31	85 −15	..	See 80, 86.	..	? Oct. 18-27 87 − 2
8	1870, Sept. 5	42 +55	..	Accurate. Meteors small and swift. ? Sub-radiant to great Perseus stream. See 71, 74.	A_{13} ? 85	
9	1870, Sept. 5	48 +41	..	Sub-radiant suspected. See 48.	$R_{1,2}$	
0	1870, Sept. 6	35 +45	..	Another sub-radiant. (D.) 1869, Aug. 10, 37°+46°.		
1	1870, Sept. 6	25 +60 40	.. ..	? 68	92. $A_{12,13}$	
2	1870, Aug. 29	(78 +23): ..				Sept. 70 +32
	1869, Sept. 8-10	78 +23 16:		See 64, 75, 79, 83.	104	Oct. 10-27 71 +31
	1871, Sept. 22	75 +15 ..				Oct. 10-27 79 +13
3	1871, Sept. 7-15	345 +13	..	(H.) Sept. 1-15, 343°+10°.	89, T_2	? Sept. 1-10 337 +20
4	1871, Sept. 7-15	54 +56 64	.. ..	?68. Weiss, 1869, Aug. 13, 70°+53°.	97, A_{15}	
5	1869, Sept. 13	68 + 5 8		Accurate.	..	Aug. 3-12 55 + 7
	Sept. 14	62 + 9 5		? 72.	..	? Sept. 3-30 51 +14
	Sept. 15	65 + 3 5		..	..	Oct. 10-22 62 + 6
6	1869, Sept. 13	68 − 5 ..		Sub-radiant to 75.		
	Oct. 12	76 −10 ..		Accurate. See 80.	..	? Oct. 18-27 87 − 2
	Oct. 13	77 −10 ..		..		
7	1871, Sept. 22	345 +61 ..		? 54.	90, 96	Aug. 7-31 347 +51
8	1869, Oct. 5-6	54 −14 ..		(N.) Oct. 50°−4°.	..	Aug. 55 −18 Sept. 55 − 6
9	1869, Oct. 5-6	91 + 9 10				
	Oct. 8	86 + 6 6		Sub-radiants.	..	Sept. 82 + 6
	Oct. 10	90 +12 9				
	Oct. 12	89 +16½ ..		Accurate.		
	Oct. 13	87½+14 ..		Accurate.	104	
	Oct. 14	87 ± 0 ..		Sub-radiant, well marked.	..	? Nov. 79 + 5
	Oct. 14	97 ÷10 ..		Sub-radiant.		

No.	Date of Observation.	R.A. and Declination for 1830.	Zenith Horary Number.	(D.), Prof. Denza (Memorie, etc.) (H.), Dr. Heis. (N.), Prof. Neumayer. (S.), Dr. Schmidt. (A.S.H.), Prof. Herschel.	Mr. Gregg's Numbers and Dr. Heis' Letters.	Dr. Schmidt (Athens) Astr. Nach. No. 1756.
79	1869, Oct. 15-16	89° +16°	12	Accurate.		
	Oct. 17	91 +18	..	Good. A.S.H. 1864, Oct. 18, 90°+16°.	..	Oct. 18-27 93+
80	1869, Oct. 5-7	85 − 2	6½	? Sub-radiant 79.		Oct. 18-27 87 −
	Oct. 12	90 −10	..			
	Oct. 14	87 ± 0	..			
81	1869, Oct. 7	39 + 3	6			
	Oct. 13	43 +11	..	Very accurate. See 83, 93.		
	Oct. 14	44 + 4	..	Good. (H.) Dec. 10-21, 41°+12°.	? 87	? Oct. 50 +
82	1869, Oct. 8	107 +12	..			
	Oct. 14	110 + 6	..	See 87.	? 105	Oct. 108 + / Nov. 113 +
	Nov. 7	101 + 7	4	Good.		
83	1869, Oct. 8	77 +37	6	(H.) 72°+44°.		
	Oct. 12	68 +25	..	Rough. See 72.	? 100	Sept. 70 +
	Oct. 13	77 +30	..	Accurate.	A_{16}	Oct. 10-27 71 +
84	1869, Oct. 10	47 − 6	8½	(N.) Oct. 50°−4°.	..	Sept. mean of three. 43 −
85	1869, Oct. 11	150 +13	13	..	..	? Oct. 140 +
86	1869, Oct. 11	93 −24	..	See 67, 80.		
	Oct. 12	93 −18	...			
87	1869, Oct. 11	107 − 2½	..	See 82.	..	? Oct. 115 −
	Oct. 16	101 ± 0	..			
88	1869, Oct. 11	128 +20	..	Suspected.	..	? Oct. 140 + / Nov. 113 +
89	1869, Oct. 12	76 −10	..	Accurate.	..	Oct. 18-27 87 −
	1869, Oct. 13	77 −10	..	See 80.	..	Dec. 102 +
90	1869, Oct. 12	105 +24	..	Birmingham, Dec. 107°+19°. See 82.	105	Dec. 10-21 111 +
91	1869, Oct. 13	58 +10½	..	Accurate.		
	Nov. 7	50 +14½	9	Accurate.		Oct. 10-22 62 +
	Nov. 9	52 +12½	8	Good.	111	Oct. 50 +
	Nov. 7	57 +19½	6	Accurate.	R_4	Dec. 55 +
	Nov. 9	59 +18	5	Good.		Dec. 58 +
	Nov. 10	53 +18	7	Accurate.		
				Rich shower with well defined double radiant, 1872, Nov. 1-3, 56°+24°.		
92	1869, Oct. 15-16	86 +45	..	(A.S.H.) 1864, Sept. 27, 85°+50°.	100. ? A_{16}	Nov. 82 +
93	1869, Oct. 13	28 +10	..	Accurate.	..	? Oct. 19-27 33 +
94	1869, Nov. 3	30 +22	..	..	? 109	Dec. 34 +
95	1869, Nov. 6	61 + 1	..	..	..	Oct. 10-22 62 + / Dec. 55 +

	Date of Observation.	R.A. and Declination for 1830.	Zenith Horary Number.	(D.), Prof. Denza (Memoire, etc.) (H.), Dr. Heis. (N.), Prof. Neumayer. (S.), Dr. Schmidt. (A.S.H.), Prof. Herschel.	Mr. Greg's Numbers and Dr. Heis' Letters.	Dr. Schmidt (Athens), Astr. Nach. No. 1756.	
6	1869, Nov. 6	57° − 9°	..	..			°
7	1869, Nov. 7	160 +40	..	Position estimated.			
8	1869, Nov. 7	124½ + 4½	..	Accurate. Sharply defined.	112	Dec.	120 +10
9	1869, Nov. 10-11	77 +10	3	..	..	Oct. Nov. Dec.	79 +13 79 + 5 73 + 4
0	1869 Nov. 13-14	151·0 + 21·5	..	*Monthly Notices*, vol. xxx. p. 29.	115	Nov. 10-14	148 +22
	1866 Nov. 13-14	149·8 + 22·2	..	Equinox of dates.	L	Dec.	146 +16
1	1870 Dec. 12	110 +40	..	Wood, 1866, 112°+34°. (H.) Dec. 112°+39°. (Bir. Dec. 12, 1867, 107°+19°.	125 M_9	Oct. Jan.	112 +48 105 +44
2	1869 Dec. 23-27	130 +49	..	(H.) December 15-31, 137°+59°. Jan. 1-15, 135°+57.	2	? Dec.	130 +30

Appendix to the Catalogue of Radiants containing the Determinations of the great August Radiant in Perseus.

Date of Observation.	R.A. and Declination 1850.	Z. H. N.	
1870 July 27–29	Perseus.		
1869 Aug. 4	39° + 58°		
Aug. 4	47·5 + 58·0		
1870 Aug. 4	45 + 60	15	
Aug. 5	54 + 54	10	
1869 Aug. 6	47·5 + 48·0		Accurate.
1870 Aug. 6	42 + 56	13	,,
Aug. 7	46 + 61	31	
1869 Aug. 8–10	50 + 56 42 + 64 50 + 63 47·5 + 58·0	26	
1870 Aug. 8	45·0 + 59	95 :	R.A. accurate, 14 meteors counted in 9½ minutes at 15h 20m.
Aug. 9	42·0 + 57·5	60	Accurate.
1871 Aug. 10	43·0 + 59·0 40·5 + 56·5	65	,, ,,
1869 Aug. 11	50 + 56		
Aug. 11	39 + 65		
Aug. 11	47·5 + 59·0	20	,,

Date of Observation.	R.A. and Declination 1850.	Z. H. N.	
1870 Aug. 11	43 ·5 + 58 ·5	10	Full moon.
1871 Aug. 11	40 ·5 + 57 ·5 40 ·5 + 56 ·0	13	Both accurate.
Aug. 11	45 + 62		
1869 Aug. 12–15	47 ·5 + 59 ·0		Sharply defined.
1871 Aug. 12	46 ·0 + 58 ·4 40 : + 56 ·5	13	Accurate. Decl. accurate.
Aug. 13–18	Perseus.		Z.H.N. = 10 on 13th
1870 Aug. 14–19	,,		Poor 14th and 16th rich 18th and 19th
Aug. 22	55 + 52	5	Accurate.
1871 Aug. 20–25	55 + 57		Decl. accurate.
1870 Aug. 29	45 + 50		
Aug 29.	75 + 45		No. 66.
Sept. 5	40 45 + 55		Accurate, No. 68, ? branch stream.
1871 Sept. 7–15	54 64 + 56		No. 74.

Other Determinations.

Authority.	Date.	Position.
Dr. Schmidt*	Aug. 3–10	46° + 55°
,,	Aug. 3–12	31 + 55
,,	Aug. 3–12	50 + 48
,,	Aug. 3–17	50 + 62
,,	Aug. 3–11	56 + 47
Dr. Heis†	July 16—Aug. 15	50 + 51
,,	July 1–15	41 + 62
,,	July 15–31	51 + 55
,,	Aug. period	51 + 55
,,	Aug. 15–31	35 + 61
,,	Sept. 1–15	35 + 63
,,	Sept. 16–30	44 + 63
,,	Oct. 1–15	51 57 + 61
Dr. E. Weiss‡	1869 Aug. 11	49·9 + 55·6
,,	Aug. 12	49·5 + 56·7
,,	Aug. 13	49·1 + 61·6
,,	1869 Aug. 11–13 mean of three	73·1 + 53·6
Prof. Schiaparelli §	1866 Aug. 10·7	41 + 56
Prof. Denza §	1868 Aug. 10	44 + 57
,,	1869 Aug. 10	44 + 56·5
,,	1869 Aug. 11	35 + 60 :

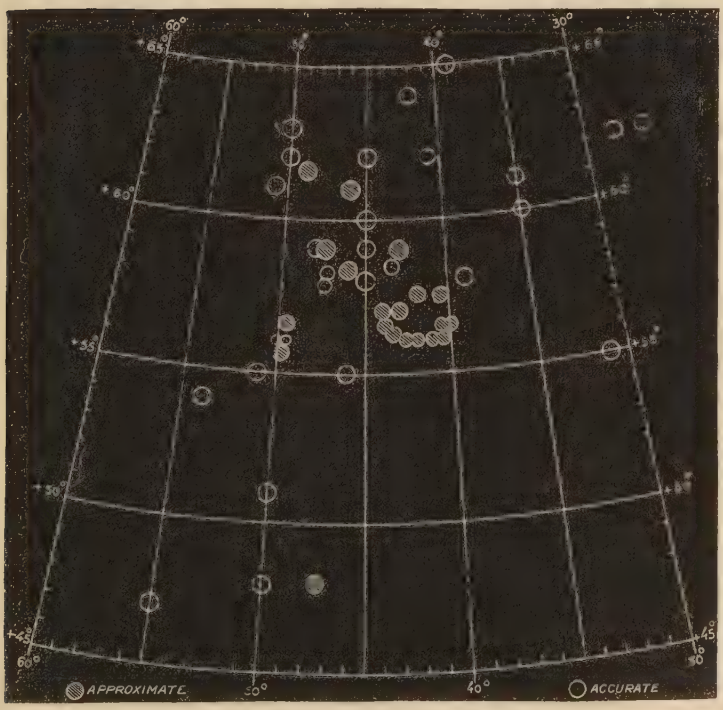
Authority.	Date.	Position.	
		°	°
Prof. Parnisetti §	1869 Aug. 10	23	+ 57
,,	,,	61	+ 43
,,	1869 Aug. 11	26	+ 57
Prof. Lorenzoni §	1869 Aug. 8–13	26	+ 62
,,	,,	58	+ 58
,,	,,	37	+ 46
Prof. Serpieri §	1869 Aug. 10	44	+ 56·5
Prof. Tacchini §	Aug. 10	43·3	+ 56·2
,,	,,	42·5	+ 56·0
,,	Aug. 11	27·8	+ 62·0
At Lodi §	Aug. 10	43	+ 57
Mr. Twining *	Aug. 10	45	+ 58

* *Astronomische Nachrichten*, No. 1756.

† *Monthly Notices*, vol. xxiv. p. 213; and B. A. Atlas for 1868.

‡ *Beiträge zur Kenntniss der Sternschnuppen*, 1870, Mai, 19.

§ *Memorie* (V. and VI.) *sulle Stelle Cadenti*. Milano, 1869?



Perseus Radiants, August 4 to 12.

General Horary Numbers.

Date and Local Mean Middle Time.	Place of Observation.	Time spent in Counting.	Number of Meteors Counted.	Horary Number.	State of the Atmosphere.
d h		h m			
1870, Jan. 2 16·5	Valletta	1 45	25	16	Clear.
4 7·7	,,	0 30	2	4	,,
4 16·0	,,	2 16	41	18	,,
5 16·9	,,	1 10	31	26	,,
7 16·0	,,	2 0	51	25	,,
8 16·7	,,	0 54	18	20	,,
9 14·6	,,	1 21	15	12	Cloudy.
11 15·1	,,	1 40	18	11	Clear.
1869, Jan. 19 12·0	35°·9 N, 17°5 E	0 24	0	0:	,,
21 18·3	36·0 N, 14·2 E	1 0	8	8	☾
26 17·2	Valletta	1 30	2	1:	☾ ☾ and cloud.
28 17·0	,,	2 0	4	2:	,,
29 16·6	,,	1 48	0	0:	,,
30 17·0	,,	2 12	4	2:	,,
31 9·1	,,	1 6	2	2	Clear.
31 16·9	,,	2 12	3	1:	☾
Feb. 1 16·8	,,	1 42	8	5:	,,
3 16·9	,,	2 0	15	8:	,,
1870, 3 15·5	,,	1 52	31	17	Clear.
1869, 4 16·5	,,	1 36	8	5:	☾
5 16·7	,,	1 25	14	10	Clear.
6 16·7	,,	0 54	4	4	,,
7 16·9	,,	1 40	12	7	,,
8 15·4	,,	0 35	11	18	,,
9 15·8	,,	0 21	6	17:	,,
10 16·5	,,	1 49	24	13	,,
1870, 10 15·5	,,	1 40	29	17	,,
11 15·7	,,	0 30	5	10:	,,
1869, 13 16·5	,,	1 35	16	10	,,
14 16·2	,,	0 30	5	10	,,
16 15·8	,,	0 47	6	8	,,
17 16·4	,,	0 42	11	16	,,
18 15·7	,,	0 48	8	10+	Cloudy.
21 16·1	,,	1 9	8	7	Clear.
1871, 21 11·7	,,	0 30	3	6	,,
1869, 22 16·5	,,	0 49	9	11	,,
23 16·6	,,	1 10	8	7	,,
24 16·4	,,	1 0	2	2	,,
26 16·5	,,	0 30	1	2	,,
1870, Mar. 1 12·6	,,	0 24	1	3:	,,

Date and Local Mean Middle Time.	Place of Observation.	Time spent in Counting.	Number of Meteors Counted.	Horary Number.	State of the Atmosphere.
d h		h m			
1870, Mar. 2 15·1	Valletta	1 19	21	16	Clear.
3 15·4	,,	1 43	26	15	,,
4 15·0	,,	0 51	15	19	Haze & cloud.
7 15·5	,,	2 34	31	12	Clear.
1869, 7 16·4	,,	0 34	17	30	,,
1870, 8 15·1	,,	1 21	25	19	,,
1869, 10 13·6	,,	0 26	0	0:	,,
1870, 10 14·8	,,	0 40	16	23	Little cloud.
11 14·5	,,	0 42	5	..	Very cloudy.
1869, 11 15·9	,,	1 2	6	6	Clear.
13 15·8	,,	0 55	4	4	,,
14 15·7	,,	0 30	1	2:	,,
15 15·9	,,	0 53	6	7	,,
19 14·6	,,	1 31	13	9	,,
Apr. 23 15·0	,,	1 0	1	1	,,
1870, 25 14·0	,,	1 24	11	8	,,
27 15·0	,,	1 22	28	20	,,
1870, April 28 12·1	,,	1 15	8	7	Clear.
May 2 14·8	Syracuse	1 22	25	18	,,
3 14·7	,,	1 26	8		,,
June 28 14·2	Valletta	1 38	37	23	,,
29 14·0	,,	1 10	22	17	,,
30 14·6	,,	1 30	15	10	,,
July 1 14·7	,,	1 12	25	21	,,
8 14·7	36°·7 N, 13°·7 E	1 2	29	28	,,
9 15·2	37 ·1 N, 13 ·7 E	0 34	11	18	,,
21 12·2	Taormina	0 49	21	26	,,
22 12·6	Valletta	0 48	28	35	,,
27 14·5	,,	1 48	61	34	,,
28 14·5	,,	1 38	65	41	,,
29 14·2	,,	1 10	40	33+	Cloudy.
30 14·5	,,	1 36	52	32	Clear.
Aug. 1 14·9	,,	1 13	39	32	,,
1869, 4 14·6	40°·8 N, 14°·3 E	1 13	37	30	,,
1870, 4 15·0	37 ·0 N, 11 ·5 E	1 0	41	41	,,
5 14·8	37 ·6 N, 9 ·7 E	1 6	31	28	Little cloud.
6 14·6	37 ·2 N, 8 ·0 E	1 42	57	36	Clear.
1869, 6 14·7	40 ·8 N, 13 ·3 E	1 42	58	36	,,
7 9·6	41 ·0 N, 12 ·0 E	1 0	15	15	,,
7 13·8	,, ,,	1 6	40	36	,,
1870, 7 15·6	37 ·3 N, 5 ·2 E	0 43	43	60	,,
1869, 8 9·2	41 ·1 N, 10 ·9 E	0 45	12	16	,,
8 14·1	41 ·4 N, 11 ·2 E	1 9	37	32	,,

Date and Local Mean Middle Time.	d	h	Place of Observation.	Time spent in Counting. h	m	Number of Meteors Counted.	Horary Number.	State of the Atmosphere.
1870,	8	15·3	37·2 N, 3·0 E	0	10	14*	88	Nearly overcast. ☾ full.
1870, Aug.	9	15·5	36·9 N, 0·3 E	1	0	12*	—	Cloudy; ☾ full.
1869,	10	13·0	43·0 N, 9·4 E	1	4	80	75	Clear.
	10	15·7	„ „	0	30	20	40	„
1871,	10	13·0	48·2 N, 5·7 W	1	49	111	61	☾ clear.
1870, Aug.	10	15·4	36·4 N, 2·8 W	1	45	2	—	Cloudy, and full Moon, but certainly no bright meteors.
1871,	11	12·7	48·7 N, 5·8 W	1	24	64	46	Clear.
1869,	11	14·5	43·0 N, 9·4 E	1	0	61	61	„
1870,	11	15·0	36·1 N, 5·2 W	1	12	16	13:	Full ☾, clear.
1871,	12	13·3	49·8 N, 4·7 W	2	6	57	27	Clear.
1869,	12	14·4	43·0 N, 7·7 E	1	9	31	27	A little cloud.
1871,	13	12·7	50·0 N, 4·8 W	1	12	20	17	Clear.
1870,	14	9·3	Gibraltar	0	24	8	20:	„
1871,	14	15·1	50·0 N, 6·0 W	0	6	3	30::	Cloudy.
1869,	15	12·6	43·3 N, 5·3 E	1	0	27	27	Clear.
1870,	16	9·5	Gibraltar	1	6	34	31	„
	18	9·9	„	0	42	13	19	Cloudy.
1871,	18	13·8	Queenstown (Cork)	0	27	5	11	„
1869,	18	14·3	Marseilles	0	24	1	—	☾ clear.
	18	15·1	„	0	52	18	21	Clear.
1870,	19	9·5	Gibraltar	0	36	15	25	„
	19	11·0	„	0	25	9	25:	„
1871,	20	13·0	Queenstown	1	4	30	28	„
	20	15·1	„	0	30	6	12	„
1870,	21	12·5	Lisbon	1	12	20	17	„
	22	13·3	„	0	30	8	16	Clear and ☾.
	23	13·5	40°·9 N, 9°·2 W	2	6	47	22	Clear.
1871,	24	12·2	Queenstown	1	30	2	—	Brilliant Aur. Bor.
	25	12·6	„	1	8	14	12	Clear.
1870,	28	15·1	Vigo (Spain)	1	30	32	21	„
1870, Aug.	29	14·4	„	0	36	11	20	Hazy.
	30	14·5	„	1	36	28	17	Clear.
	31	14·9	„	1	3	30	28	„
Sept.	5	14·9	37°·0 N, 9°·2 W	1	18	37	28	„
	6	15·4	Cadiz	1	0	25	25	„
1871,	7	11·6	Lisbon	1	34	29	19	„
1869,	8	13·9	Cape St. Vincent	1	34	29	18	„
1871,	9	12·1	Lisbon	0	42	12	17	Little cloud.

* All very bright meteors.

Date and Local Mean Middle Time.		Place of Observation.	Time spent in Counting.	Number of Meteors Counted.	Horary Number.	State of the Atmosphere.
d	h		h m			
1869, Sept. 10	14·0	,,	1 0	18	18	Thin cloud.
13	14·9	,,	1 30	34	23	Clear.
14	15·1	Lisbon	1 7	21	19	,,
1871, 15	13·5	,,	0 33	8	14	Cloudy.
1869, 15	14·7	,,	1 16	24	19	Little haze.
1871, 21	15·2	,,	0 50	9	11	Clear?
1871, 22	13·8	,,	1 12	8	7	Cloudy.
1869, Oct. 2	11·4	36° 2 N, 3°·3 W	1 16	27	22	Clear.
4	14·1	36 ·5 N, 1 ·4 W	0 57	21	22	,,
4	15·3	,, ,,	0 49	25	30	,,
5	14·4	36 ·6 N, 0 ·1 W	1 9	17	15	,,
6	14·2	37 ·2 N, 1 ·0 E	0 44	19	26	,,
6	15·3	,, ,,	0 42	12	17	,,
7	14·6	Algiers	1 4	13	12	,,
8	14·2	37°·3 N, 5°·1 E	0 46	14	18	Cloud.
8	15·2	,, ,,	0 36	21	35	,,
10	14·5	37 ·5 N, 10 ·0 E	0 58	23	24	Little cloud.
11	15·4	36 ·5 N, 12 ·0 E	0 50	24	28	Clear.
12	13·8	36 ·3 N, 13 ·8 E	0 41	25	35	Little cloud.
12	15·0	36 ·2 N, 13 ·9 E	0 44	26	36	Clear.
13	13·0	Valletta	0 41	26	38	,,
13	14·2	,,	0 48	26	32	,,
13	15·5	,,	0 37	28	45	,,
14	13·8	,,	0 38	31	48	,,
14	14·9	,,	0 37	28	44	,,
14	16·0	,,	0 30	32	64	,,
15	15·5	,,	0 55	50	54	,,
15	16·5	,,	0 31	41	80	,,
16	16·0	,,	0 35	47	81	,,
17	16·7	,,	0 35	24	41	Thin cloud.
28	10·7	,,	0 40	12	18	Clear.
29	12·9	,,	0 13	7	33:	,,
Nov. 3	13·0	,,	0 28	15	32	Very little cloud.
3	14·0	,,	0 37	24	39	,,
3	15·3	,,	0 31	16	31	,,
3	16·3	,,	0 34	21	37	,,
6	13·1	33°·7 N, 21°·4 E	0 25	16	38	Clear.
6	13·8	,, ,,	0 29	17	35	,,
6	14·5	,, ,,	0 34	15	27	,,
7	13·2	32 ·3 N, 24 ·2 E	0 34	26	46	,,
7	14·3	,, ,,	0 36	26	43	,,
7	15·3	,, ,,	0 39	18	28	,,

Date and Local Mean Middle Time.			Place of Observation.	Time spent in Counting.	Number of Meteors Counted.	Horary Number.	State of the Atmosphere.
	d	h		h m			
1869, Nov.	9	13·2	31 ·4 N, 29 ·6 E	0 36	10	17+	Cloudy.
	9	14·4	,, ,,	0 30	13	26	Clear.
	9	15·4	,, ,,	0 25	11	26	,,
	10	8·1	Alexandria	0 55	9	10+	Cloud and ☾.
	10	13·5	,,	0 48	17	22+	Cloudy.
	10	14·5	,,	0 36	18	30+	,,
	10	15·5	,,	0 42	14	20+	,,
	11	8·4	,,	0 15	6	24+	Cloud and ☾.
	11	13·4	,,	0 48	14	17	Clear.
	11	14·6	,,	0 40	10	15	,,
	12	14·1	,,	1 9	28	25+	Cloud.
	13	15·5	Port Saïd	1 16	112*	—	Very cloudy.
	14	15·0	,,	0 54	5	5	Cloudy.
1870, Dec.	12	7·9	Valletta	1 0	13	13+	Clear.
	14	7·2	,,	2 0	7	4	,,
	15	9·2	,,	1 12	6	5	,,
	16	9·2	,,	2 12	7	3	,,
	17	9·0	,,	3 0	6	2	,,
1869,	23	13·1	,,	0 38	15	19	☾ ☾ and cloud.
	27	11·6	,,	1 30	9	6+	Very cloudy.

Some Observations of the Colours and Magnitudes of Southern Stars in the Year 1864. By Capt. G. L. Tupman, R.M.A.

When at Montevideo, in May and June 1864, I proposed to register the colours of all stars, visible to the naked eye, within 60° of the South Pole, and to make an attempt to compare their magnitudes with those obtained by the late Sir John Herschel, in 1836–8. I was, however, ordered home shortly after commencing, and only lately have thought the few observations made worth communicating, chiefly on account of the number of coloured stars observed.

The magnitudes have been obtained from observed sequences by plotting on charts of engraved squares, using Sir John's sequence magnitudes as ordinates, and drawing a free curve through all the points obtained, by which method the new magnitudes are strictly comparable with those formerly obtained.

In the Catalogue the Constellations are arranged alphabetically, and the stars in the order of lettering for convenience of reference. The letter and the B.A.C. number are considered sufficient for identification. The magnitudes given in the Cape "Results" are entered in the third column under 1837. Among the Notes are occasional observations with an achromatic of

* All very large meteors. See *Monthly Notices*, vol. xxx. p. 29.

9½-inches aperture, by Fitz, of New York, then the property of W. G. Lettsom, Esq., Her Majesty's Consul-General at Montevideo, who most kindly allowed me the use of it.

The following abbreviations have been employed,—

v.	very	$= \alpha$	Equal to α, brighter than β, less than γ (*of the same constellation* unless otherwise indicated).	n.	north
bt.	bright	$> \beta$		s.	south
or.	orange	$< \gamma$		p.	preceding
lit.	little			f.	following
sm.	small				
D.	double				
m.	magnitude				

Colours and Magnitudes of Southern Stars, 1864, *May and June.*

Ara.

	B.A.C.	1837. Mag.	Mag.	1864. No. of Obs.	Colour.	Notes.
α	5899	3·40	3·50	2	..	$= \zeta$
β	5852	3·31	..	..	Deep bt. orange	$> \gamma\, \delta\, \zeta$
γ	5850	3·82	..	..	Bluish white	$< \zeta, > \delta$
δ	5877	..	..	..	Pale orange	$< \gamma > \eta$
ζ	5683	..	3·50	2	Bright orange	$= \alpha$, between β and γ in Mag.
η	5607	..	..	..	Orange	$< \delta < \gamma$

Argo.

α	2096	0·22	..	..	White	Brighter than Arcturus
β	3177	2·03	2·30	2	,,	$= \varepsilon = \gamma, > \iota\, \varepsilon\, \delta$
γ	2755	2·08	2·15	3	,,	lit. $> \iota = \varepsilon\, \beta$
δ	2979	2·42	2·45	5	,,	$= \iota\, \lambda$, lit. $< \gamma$, lit. $> \varkappa$
ε	2832	..	2·25	5	Yellow; orange	$= \beta\, \gamma$
ζ	2710	2·72	2·77	3	..	$= \varkappa$, nearly $= \nu$ Scorp.
η	3695	..	5·41	8	Orange	
θ_2	3686	3·26	3·26	4	..	$= \alpha$ Musc. $= \delta$ Cruc. Has brilliant loose cluster about it
ι	3186	2·80	2·71	5	White	$= \delta\, \lambda$, lit. $> \varkappa$, v. lit. $< \gamma$
$\varkappa$	3213	2·94	2·95	4	,,	$= \zeta$, nearly $= \nu$ Scorp. $< \iota\, \delta$ Arg.
λ	3126	2·46	2·43	3	Orange	$= \iota\, \delta$
μ	3702	3·08	3·02	5	Very red	
ν	2188	3·74	3·69	3	..	$> \delta$ Cruc. $> \alpha$ Musc. $> \theta$ Arg.
ξ	2602	3·74	..	..	Yellow	Three or four stars *sp.* largest of which is white
o	2950	3·99	4·18	2	White	A cluster

	B.A.C.	1837. Mag.	Mag.	1864. No. of Obs.	Colour.	Notes.
π	2414	2·98	..	..	Very rich yellow	Two stars *n f* both white; 9 or 10 sm. stars in field.
ρ	2728	3·32	..	..	Yellow	
σ	2482	3·79	..	..	Yellow	
τ	2256	3·50	..	..	Light yellow	$> \sigma$
υ	3565	3·53	3·40	2	White?	= ór lit. $> \omega$
φ	3410	4·33	4·22	2		
χ	2665	4·03	4·27:	1	..	= *a* Carinæ, < N Car.
ω	3516	3·72	3·8	1	White	= or $< \upsilon$

Carina Argûs.

a	3149	4·35	4·27	3	White	= χ Arg. $>$ *c i g* Car. < N Car.
b_1	3073	..	4·7?	1	,,	$b_1 = b_2 < f$ Car.
b_2	3089	..	4·7?	1	,,	
c	3064	..	4·40	2	,,	= *i*, < *a*, > *u*
e_1	2921	..	4·4?	1	,,	$e_1 = e_2 = f h l$
e_2	2920	..	4·4?	1	,,	
f	2998	..	4·4	1	,,	= *h l*
g	3179	..	4·6?	1	Deep orange red	= *m*, < *a*. In the 9-inch this star is a very rich yellow
h	3289	..	4·4	1	White	= *f l*
i	3152	..	4·35	2	..	= *c* = o Arg.
l	3353	4·38	4·36	2	Yellow; orange	= *a f h* < o Arg.
m	3320	..	4·6?	1	White	= *g* < *h l*
p	3619	3·90	3·88	3	,,?	= *q*, lit. < *q*
q	3526	3·85	3·93	3	Orange red	= *p*
r	3635	..	4·94	7	Orange red	
s	3594	..	4·27	7	Yellow; orange	< *u*, > X Vel.
t_1	3462	..	5·98	7		
t_2	3655	..	5·23	7	Deep or. red	Bris. 3127, cluster 6 mag.
u	3740	4·51	4·19	7	Bt. or. red	> *s*, < *c*
x	3818	4·83	4·27	3	Deep or. red	
N	3269	3·69	3·60	4	Yellow; orange	> *a*, > υ Arg. > χ Arg. In the 9-inch a very rich yellow
P. Vel.	3589	..	5·50	6	White	Variable?
T ,,	3546	..	5·1	1	,,	
V ,,	3536	..	5·50	2	Red	
X ,,	3658	..	4·41	7	Yellow?	< *s*; Bris. 3135 double.
Z ,,	3705	..	5·94	7	White	Variable; 6·5 in 1872
—	3680	..	6·33	5	..	Brisbane *red*

	B.A.C.	1837. Mag.	Mag.	1864. No. of Obs.	Colour.	Notes.
—	3688	..	5·29	7	Deep or. red	„ red
k Pup.	2530	..	..	..	Yellow?	A coarse double star, components and alike
11 „	2652	..	..	..	Light yellow	
72 „	2827	..	..	..	Orange	
—	3624	..	..	..	Deep orange	= K Car.

Canis Major.

δ	2345	2·32	..	..	Fine yellow	
η	2458	2·85	..	..	Purple?	Not white
ι	2274	..	..	..	Light yellow	Small star *f* which is very red
λ	2066	4·16	..	..	Rich or. yellow	
o_1	2267	4·36	..	..	Intense yellow	Small star *np* quite red; two small stars in field which are white. Fine object
o_2	2318	3·75	..	..	Rich yellow	Small star *np* deep orange; near four or five others which are white
π_2	2269	..	..	..	Dull yellow	
σ	2309	3·92	..	..	Rich or. yellow	
27	2388	..	..	..	White	Small star *s* full orange

Centaurus.

α	4832	0·34	..	..	Rich yellow	= Arcturus, or brighter
β	4669	1·14	1·20	3	White	= α Cruc. = Ant., > Spica.
γ	4264	2·68	2·70	4	„	= ι Scorp. scarcely < θ Cent. < γ Cruc. Just divided with 220 in 9-inch
δ	4087	2·99	3·07	2	White	= $\eta\,\zeta$, scarcely less ε
ε	4549	2·82	2·89	4	„	Exactly = α *Lupi*
ζ	4638	2·96	3·00	4	„	= or > $\eta\,\delta$ scarcely < ε
η	4811	2·91	2·89	4	„	= $\delta\,\zeta$, nearly = ε
θ	4626	2·54	2·57	2	Pale orange?	v. lit. < λ Scorp.
ι	4458	3·20	3·2	1	White	lit. > $\varkappa$ ζ Lupi
$\varkappa$	4928	3·60	3·42	3	„	= π Scorp.
λ	3941	3·70	3·6	1	„	= η Car. = β Musc. Between this star and B.A.C. 3986 is a fine curve of 6 m stars, one of which is the cluster *h*. 3352

	B.A.C.	1837. Mag.	Mag.	1864. No. of Obs.	Colour.	Notes.
μ	4602	3·93	3·66	3	White	
ν	4601	3·95	3·95	2	,,	= ε Lupi, nearly = δ Lupi
ν_1	4654	4·60	4·38	2	,,	= ι Lupi
ϱ	4103	4·63	4·6	1	,,	
φ	4653	4·54	4·5	1	,,	
—	4000	..	..	..	,,	= $\eta\ \zeta$ Cruc.

Crux.

α	4187	1·2	1·20	3	White	= β Cent. > Spica, > Antares
β	4289	1·6	1·51	4	,,	= Spica, Antares
γ	4215	1·73	1·69	4	Orange	Nearly = β. Have sometimes thought it > β
δ	4120	3·57	3·35	2	White	= α Musc. = θ Arg.
ε	4158	4·61	4·3?	1	Orange	> o Cruc. < λ Cent.
ζ	4133	4·71	..	..	White	= $\eta\ \theta$, B.A.C. 4000
η	4078	4·60	..	..	,,?	= ζ
o	4327	4·61	..	..	,,	= ε, h. calls it o Crucis
θ_1	4061	..	..	..	,,	= $\eta\ \zeta$

Lupus.

α	4839	2·82	2·87	3	..	= ε Cent.
β	4924	3·14	3·11	4	..	Nearly = $\zeta\ \delta\ \eta\ \varepsilon$ Cent.
γ	5118	3·36	3·35	3	White	
δ	5046	3·94	3·95	2	,,	
ε	5056	4·00	3·95	1	,,	= ν Cent.
ζ	4987	4·11	4·4	1	,,	= $\varkappa$, < $\iota\ \upsilon_1$ Cent.
ι	4734	4·16	4·3	1	,,	= υ_1 Cent.
$\varkappa$	4986	4·32	4·4	1	,,	= ζ

Musca.

α	4245	3·43	3·42	2	White	= δ Cruc. = θ Arg.
β	4280	3·67	3·50	2	,,	= exactly λ Cent.
γ	4224	4·67	4·8?	1	..	< δ, < B.A.C. 3984
δ	4353	4·60	..	..	Yellow	= B.A.C. 3984, < β, > γ
	4129	..	..	..	Orange red	Two stars 6 m make right angle at ε, both white
	3984	..	4·6:	1	White	= δ
	3993	..	..	..	Red	

Scorpio.

	B.A.C.	1837. Mag.	1864. Mag.	No. of Obs.	Colour.	Notes.
α	5498	1·28	1·2	1	Orange	Companion easy with 3-in. aperture
γ	6018	3·98	..	..	White	$= \iota_1$
ε	5632	2·71	..	..	Orange; yellow	$= \varkappa$
θ	5395	2·29	2·3	1	White	$= \lambda$
ι_1	6004	3·53	..	..	„	$= \gamma$ Cent. D star f 5′; 11, 13; 2″
$\varkappa$	5970	2·91	..	..	„	$= \iota$, $> \upsilon$
λ	5915	1·87	2·05	1	„	$= \theta$, nearly $= \theta$ Cent.
μ_{1+2}	5638	μ_1 3·67 / μ_2 4·16	3·60	1	„	$\mu_1 = \mu_2$!
υ	5382	3·42	3·0	1		h. has another value. 4·58
π	5289	3·35	3·65	1		$= \varkappa$ Cent.
σ	5447	3·50	3·5	1		$= \beta\gamma$ Triang. Aus.
τ	5539	3·44	3·5	1	„	$= \sigma$

Triangulum Aust.

α	5578	2·23	2·2	1	Orange; yellow	2·7 rejected
β	5233	3·46	3·57	3	White	
γ	6255	3·51	3·60	2	„	
ε	5103	..	..	..	Orange	
Spica		1·41	1·2	1	White	$= \beta$ Centauri
α *Circini*		3·78	3·9	1		
α *Pictoris*		3·77	3·7	1		

On the supposed Re-discovery of Biela's Comet. (No. 2.)
By Captain Tupman.

The letter of Professor Klinkerfues in the last number of the *Monthly Notices*, combined with the now well-known singular circumstances attending Mr. Pogson's observations, naturally leads all those who have not gone into the matter geometrically into the belief that the long-lost Comet of Biela has been really seen again.

The lines of sight, or "directions of observation," on December 2 and 3, pass at such a distance to the north of the known orbit of Biela's Comet that accurate heliocentric co-ordinates of the comet's position in space cannot thereby be obtained.

As much depends upon the actual orbit in which the meteors of November 27 were moving, I have computed the elements from the observed position of the radiant point on the assumption of a

periodic time similar to that of the Comet. The radiant point given by Professor Alexander Herschel in the *Monthly Notices*, vol. xxxiii. p. 75, which is probably the most accurate, is in

Longitude	40 47′	App. Equinox 1872·9.
Latitude	+ 30 35	

and adopting this, I find the following elements of the orbit for the same equinox :—

Perihelion Passage, 1872, December 26·90

Longitude of Perihelion (π)	111° 48′
,, Ascending Node (Ω) ...	245 57
Inclination (I)	13 24
Perihelion Distance (q)...	·8265
Eccentricity (ε)	·7670
Motion	Direct.

The elements of Biela's Comet are (for same equinox) :—

P.P.	1872, October 6·4 (?)
π	109° 24
Ω	245 54
I	12 34
q	·8718
ε	·7600
Motion	Direct.

An alteration of less than 1° in the position of the radiant point would have made all the elements of the meteor-orbit absolutely identical with those of the comet, whereas a considerable alteration of the assumed periodic time would have had an almost inappreciable effect. Hence the meteors were certainly moving in the very same orbit as the comet was moving in at its former appearances.

Now the coincidence of these two sets of elements is of the utmost importance in the matter under consideration. It proves that the perturbations of the node, inclination and radius vector of a body moving in the same orbit as the comet, and twelve weeks behind, are (in this instance) sensibly the same as those of the comet itself; also that an exceedingly near approach to the Earth cannot produce an appreciable disturbance of the orbit elements.

These facts bear upon Mr. Pogson's observations in the following manner. If the comet were really twelve weeks behind time, and passed close to the Earth on November 27·33, as

assumed by Professor Klinkerfues, it must have appeared afterwards in these positions :—

	Geocentric Longitude.	Latitude.
d	° ′	° ′
1872, Dec. 2·5086	215 18	− 29 30
3·5030	216 36	− 29 13

And if the nucleus be supposed to be a little in advance, its apparent place will be somewhat *behind* on the line joining these two positions prolonged backwards ; that is, with somewhat less longitude and greater south latitude.

Mr. Pogson's positions, expressed in ecliptic co-ordinates, are,—

Dec. 2·5086	221° 50′·7	− 20° 31′·0
3·5030	224 56·6	− 19 45·0

The discordances of the longitudes, considering each by itself, may be explained by supposing a further retardation of the Comet; but the other discordances are not to be accounted for by any reasonable alteration of the circumstances. The discordance in absolute latitude would be the result of advancing the node 3°, of shortening the radius vector by ·033°, of reducing the inclination to 7½°, or of partially effecting two or more of these changes ; but it has been shown that with these elements in particular no such alterations can be admitted, not even to the extent necessary if all three were simultaneously disturbed.

Without going any farther, it is obvious that the same body moving in an orbit parallel to that of the meteors could not have been seen in *both* the Madras positions. By an alteration of the place of the node, it might have been in either of them, but with an apparent motion *per diem* of + 1° 18′ in longitude, and + 0° 17′ in latitude. On the supposition of *two* bodies having been seen, they would each have this apparent motion. Now Mr. Pogson's letter contains internal evidence, amounting to positive proof, that he saw one and the same comet on both occasions, for the observed motions in R.A. of 2ˢ·5 in 4 minutes, and of 17ˢ·9 in 28 minutes, are both in perfect agreement with the total change of 15 minutes in 24 hours. The actual change of longitude was therefore + 3½°, and of latitude + 0° 46′ in 1 day. This was pointed out to me by Professor Herschel after my last communication on the subject. Such apparent motion could only exist in an orbit differing essentially from that of the meteors.

Since, therefore, it cannot be admitted that Mr. Pogson observed two bodies, the only conclusion to be drawn is, that the one he did see was neither Biela's Comet nor a meteoric aggregation travelling in the same orbit, nor a body that had passed anywhere near the Earth on November 27, or subsequently, in spite of the extraordinary circumstances attending its discovery.

Portsmouth, Feb. 25, 1873.

On the actual state of Calculations with reference to Biela's Comet. By J. R. Hind, Esq. F.R.S.

At the suggestion of the Astronomer Royal, I submit to the Society a brief account of the present state of calculation as regards the perturbations of Biela's comet, a subject which has lately engaged my attention, and possesses interest in connexion with the presumed close approach of the Earth to one of the bodies now forming Biela, during the remarkable shower of meteors on the night of November 27th, 1872.

It is well known that both nuclei of the comet were last observed in the autumn of 1852, having been found much further from their calculated places than was expected, a circumstance which undoubtedly affected the number of observations, and which was occasioned by the unfortunate substitution by Professor Santini of a semi-axis major depending wholly upon the observations of the previous appearance in 1846, in place of that which he had deduced from observation in 1832, and carried forward by pertubation to 1846: this source of error in the prediction for 1852 is indicated by Professor Santini in a communication made to the Venetian Institute in November 1854.* There is no reason to suppose that any perturbations beyond those resulting from known causes, operated between the appearances of the comet in 1846 and 1852, indeed the observations of these years have been connected without difficulty, by the application of planetary perturbations during the interval.

The computation of the perturbations by *Jupiter* from 1852 to 1859, was undertaken by Professor Santini, whose results are printed in Vol. IV. (3rd. series) of *Atti dell' Istituto Veneto di Scienze*, &c. In publishing his earlier researches relating to this comet, from 1826 to 1852, Professor Santini has given the numerical details *in extenso*, and thus the various steps can be readily followed. This is not the case as regards the perturbations between 1852 and 1859, but while every confidence would be placed as before on the calculations of this veteran and highly honoured astronomer, whose predictions for 1846 were so strikingly, or, as Professor Challis observed at the time, so "marvellously" verified, we have two independent series of computations which completely confirm the accuracy of his work. Dr. Clausen, the present director of the observatory at Dorpat, has carried on the elements by the application of the perturbations of *Jupiter* from 1852 to 1866. (*Mélanges Mathématiques et Astronomiques* from *Bulletin de l'Acad. Imp. des Sciences de St. Petersbourg*, t. iii), and the late Professor Hubbard, of the United States' Naval Observatory at Washington, in the course of his laborious and masterly investigations on the movements of Biela's comet

* "Rendesi cosi manifesto, quanto sarebbe stato opportuno ritenere l'antico asse maggiore, il quale avrebbe condotto a posizioni molto più vicine al vero &c." *Sulla cometa periodica detta di Biela, lette all' I. R. Istituto Veneto. &c.*, 26 *Nov.* 1854.

(Gould's *Astronomical Journal*, vols. iii. vi.) "computed by Encke's method and in all strictness the perturbations of the rectangular ecliptical co-ordinates to the tenth place of decimals, from the perihelion in 1846 to April 17th, 1858, the close of the principal division of the work, and the epoch when it becomes necessary to proceed by shorter intervals." Time being wanting for the remainder of the calculation, and "as the amount of perturbation from this date to perihelion would doubtless be no greater than the unavoidable error of elements," he merely added on the motion of mean anomaly from that point for the purposes of prediction in 1859. His results thus derived appear in vol. v. p. 187, of the above-named periodical, and from them I have inferred the times of perihelion passage of the two nuclei. It may be mentioned that in this work Professor Hubbard assumed that the *principal* nucleus in 1846 was identical with that which *preceded* in 1852; Clausen, Santini, and Michez, appear to have adopted the alternative hypothesis; and at a subsequent period Hubbard found reason to conclude that the point could not be definitively settled by the few observations of each nucleus in 1852.

The following are the times of perihelion passage in 1859, according to these astronomers, reduced to the meridian of Greenwich. The effect of perturbations produced by *Saturn* was added by Dr. Michez to Professor Santini's result depending on the attraction of *Jupiter* alone.

S. F. Nucleus of 1852, May 23·5559		Santini and Michez.
Nucleus I.	22·8149	Clausen.
Nucleus II.	24·0910	Clausen.
Nucleus I.	23·0816	Hubbard.
Nucleus II.	24·4353	Hubbard.

For the next revolution, 1859–66, we have Clausen's results already mentioned and complete details of the calculation of perturbations from *Jupiter*, *Saturn*, the Earth and *Venus* by Dr. Michez, now Director of the Observatory of Bologna, published in vol. x. (3rd series) of *Atti dell' Istituto Veneto di Scienze*, &c. Reducing as before to the meridian of Greenwich, the times of perihelion passage in 1866 are:—

S. F. Nucleus of 1852, January 26·3834		Michez.
Nucleus I.	25·5835	Clausen.
Nucleus II.	27·4607	Clausen.

It will be evident from the above figures, that allowing for known causes of perturbation the perihelion passages have been correctly fixed to about 1859, May 24, and 1866, January 26. In 1859 the position of the comet in the heavens rendered its discovery almost hopeless, and its having passed by us unobserved is thus accounted for; but it is not so as regards the return in

1866. I believe it is certain that the comet did not pass its perihelion in that year within several weeks of the time predicted. The vigorous, though unsuccessful endeavours which were made to recover the comet in the latter part of 1865 and beginning of 1866, will be in the recollection of every observer. I may, however, state here that the comet was sought for by Mr. Otto Struve on three or four nights in each month from September to the end of the year, employing the great refractor at Poulkova (*Jahresbericht am 20 Mai 1866 dem Comité der Nicolai-hauptsternwarte abgestaltet*, &c. p. 18); by Professor D'Arrest with the fine refractor of the Copenhagen Observatory on more than twenty nights commencing in August 1865, the sweeps being so extended that he expresses his conviction the comet could not have passed its perihelion within ± 8 days, from the predicted date (*Ast. Nach.* 1567); by Professor Secchi, with the Merz refractor of the Collegio Romano, who used such care in the search that he discovered many faint nebulæ not included in the catalogues of the Herschels (*Ast. Nach.* 1571); by Professor Bruhns, at Leipsic, on at least twenty nights, as well after as before the expected time of perihelion (*Ast. Nach.* 1600); by Dr. Weiss, at Vienna, on every fine night from November to February (*Ast. Nach.* 1577). In this country, also, numerous telescopes were engaged in the search. Mr. De La Rue, in particular, with his powerful reflector extended his scrutiny over a wide space on each side of the predicted positions; details of his sweeps having been communicated to me at the time in letters which I have before me, and which are conclusive that the comet could not have arrived at perihelion within many days of the time expected. Mr. Barber, of Spondon, Derby, using an 8-inch refractor, was also in constant search for the comet; and I may say the same in my own case, with Mr. Bishop's 7-inch object-glass, though, being well aware of the limits within which known causes of perturbation were likely to affect the motion of the comet between 1852 and 1866, I rather attributed the want of success in detecting it to its extreme faintness than to errors in the predicted places, and for this reason gave most attention to that precise part of the sky.

So far as I know at present, the calculation of perturbations from 1866 to 1872 has not been undertaken by any one. The subject is alluded to in *Bericht über die zweite Versammlung der Astronomische Gesellschaft;* but it has probably been felt to be a useless labour to carry forward the elements from the predicted time of perihelion in 1866, considering the want of success attending the endeavours to find the comet in the corresponding track. I find with the elements of 1866 the nearest approach of the comet to the planet *Jupiter* would take place in November of that year, being about 4·57 times the Earth's mean distance, while it might undergo rather long-continued perturbation from *Saturn* in approaching and passing the aphelion, though, as the least distance would not be less than 5·6, no great effect on the time of revolution would be likely to result.

Now as regards the meteoric shower of November 27th, 1872, it has been assumed by Professor Klinkerfues, and perhaps generally, that one of the bodies forming Biela's comet was close upon the Earth at its descending node on the evening of that day; this implies, with the latest elements of Biela, that the comet was coming down to a perihelion passage soon after midnight on December 27, the arc of true anomaly between the descending node and perihelion being traversed in 30·3 days. The elements of 1866 would indicate (perturbations of course neglected) that the comet might arrive at perihelion about the end of the first week in October, leaving about nine weeks' detention to be accounted for. This very greatly exceeds the probable effect of known causes of perturbation during the last revolution.

If we suppose that the comet did really encounter the Earth in descending to perihelion passage on December 27th, there will be found since 1852 three mean revolutions of 6·754 years, and the perturbations being small from 1866 to 1872 the comet might have been in perihelion about March 28th, 1866, instead of January 26. This circumstance and the non-discovery of the comet in that year would point to perturbation from some unknown cause between September 1852 and 1866, by which the comet's period had been lengthened. If it arrived at perihelion on March 28th in the latter year, its course, supposing the other elements not materially changed, would have been as follows :—

0^h Greenwich.	R.A.	Decl.	r	Δ	$\frac{1}{r^2\Delta^2}$
1865, Dec. 28	341°·7	+1°·5	1·574	1·669	0·145
1866, Jan. 7	346·4	2·1	1·470	1·659	0·168
17	351·7	3·0	1·367	1·636	0·200
27	357·7	4·1	1·266	1·600	0·244
Feb. 6	4·5	5·5	1·169	1·552	0·304
16	12·2	7·0	1·079	1·492	0·385
26	20·8	8·6	1·001	1·423	0·493
March 8	30·4	10·2	0·939	1·348	0·624
18	41·1	+11·6	0·898	1·271	0·767

When the comet first became visible with the Northumberland equatoreal of the Cambridge Observatory at the end of November 1845, the intensity of light was 0·55, and on the above supposition as to time of perihelion passage,* it would not attain this degree of brightness till the first week in March, when the search for the comet had about terminated.

It is clear therefore that if the perihelion passage of Biela's comet took place in 1866, six or eight weeks later than anticipated, its having passed unobserved need occasion no surprise.

* On the same hypothesis, Tempel's Comet (of the November meteors) would have about the same declination as Biela on January 14 in about 18 minutes (time) greater R.A.

Note on the approaching Reappearance of Brorsen's Comet of Short Period. By J. R. Hind, Esq. F.R.S.

The last return to perihelion of the comet discovered by M. Brorsen, at Kiel on 1846, February 26, was very closely predicted through the able computation of Professor Bruhns, his assigned epoch appearing to have been only one hour in error. It is clear there can be no considerable amount of perturbation during the present revolution. At the last perihelion passage, 1868, April 18·4, the comet's distance from *Jupiter* was 5·29, and would increase until near the aphelion passage, when it was about 10·34, after this it would diminish until about the time indicated by Professor Bruhns' orbit of 1868 for next perihelion, or 1873, October 11·0, when the distance from *Jupiter* is 5·71. As regards *Saturn*, the perturbations may be nearly as large as they can ever be from this planet's attraction while the comet retains its present orbit, but the following distances show that his influence will not be material.

1870, March 31	Comet from Saturn	5·569
June 29	„	5·475
Sept. 27	„	5·426
Dec. 26	„	5·417
1871, March 26	„	5·452

The computation of an ephemeris for the present year is announced to have been again undertaken by Professor Bruhns; the following rough places, obtained on the assumption that the perihelion passage occurs 1873, October 11·0, may perhaps possess interest in anticipation of the publication of his accurate results, and considering the small amount of perturbation during the actual revolution, should approximately indicate the comet's track in the heavens. It will be seen that the discovery should be soonest and most readily affected in the Southern observatories.

Co-ordinate Constants for Apparent Equinox, Oct. 11.

$$x = [0\cdot27977] \sin (E + 221^\circ\ 42'\cdot5) + 1\cdot02392$$
$$y = [0\cdot42166] \sin (E + 113\ 17\cdot1) - 1\cdot95988$$
$$z = [0\cdot19247] \sin (E + 71\ 30\cdot3) - 1\cdot19372$$

0^h G.M.T.	R.A. h m	N.P.D. ° ′	Δ	$\frac{1}{r^2 \Delta^2}$
1873, July 23	3 49·7	100 34	1·456	0·21
28	4 8·3	99 54		
Aug. 2	4 28·3	99 9	1·310	0·32
7	4 49·7	98 20		
12	5 12·7	97 24	1·184	0·48
17	5 37·3	96 20		

0h G.M.T.	R.A. h m	N.P.D. ° ′	Δ	$\frac{1}{r^2\Delta^2}$
1873, Aug. 22	6 3·5	95 8	1·086	0·71
27	6 31·4	93 47		
Sept. 1	7 0·8	92 18	1·023	1·04
6	7 31·6	90 42		
11	8 3·7	89 2	1·002	1·45
16	8 36·8	87 22		
21	9 10·6	85 48	1·031	1·84
26	9 44·7	84 26		
Oct. 1	10 18·8	83 21	1·102	2·08

Sweeping Ephemerides for Tempel's Comet of Short Period. (1867 II.)

(*Communicated by Mr. Bishop.*)

This comet was discovered by M. Tempel at Marseilles on the 3rd of April, 1867, and found by several calculations to be moving in an ellipse with a period of between five and six years. Mr. Searle's orbit in *Ast. Nach.* 1659, is founded upon observations between April 12th and July 24th, and when compared with a normal place deduced from the later observations, by Mr. Julius Schmidt at Athens (August 18th, 19th, and 21st) shows differences of only + 1ˢ·08 in R.A. and + 14″·0 in N.P.D. Mr. Searle's elements, which must therefore be very near the true ones for this apparition, are as follows :—

Perihelion Passage, 1867, May 23·7530 M.T. at Berlin.

Longitude of Perihelion	236° 2′ 34″·3 ☊	M. Equinox
Longitude of Ascending Node	101 12 49·9 ☋	1867·0.
Inclination	6 23 38·3	
Angle of Excentricity ..	30 30 25·3	
Log. Semi-axis Major ..	0·5014326	
Mean Daily Motion ..	627″·8535 Direct.	

Some time since Mr. Hind had remarked that in the course of the present revolution the comet near its aphelion must approach within 0·4 from the planet *Jupiter*, and it was proposed to determine as accurately as practicable the effect of this close proximity upon the time of next perihelion passage. Before this work could be commenced, it was, however, announced in the publication of the *Astronomische Gesellschaft*, that the comet would be taken in hand at the Observatory of Leipsic, and the projected calculation of perturbations in a rigorous form was consequently relin-

quished. Without perturbation, according to the above orbit the comet would have again reached its perihelion in the middle of last January, and this time having arrived without any intimation of the results of the work undertaken at Leipsic, it was determined to make an *approximate* calculation of the effect of the attraction of *Jupiter* on the length of the present revolution. The whole of the computation has been performed by Mr. W. E. Plummer, of Mr. Bishop's Observatory, Twickenham. They were continued to 1871, November 17th, at which time the perturbations were becoming comparatively light. The elements had been four times corrected between the previous perihelion passage and this date, the corrected elements being employed in the calculation of co-ordinates, &c., during the following interval.

On November 17th, 1871, the elements of the comet's orbit were found to be approximately, as subjoined :—

Longitude of Perihelion ..	238° 11′·1	From the
Longitude of Ascending Node	74 6·1	Mean Equinox.
Inclination	9 12·6	
Log. Excentricity.. ..	9·66714	or $\phi = 27° 41''·3$.
Log. Semi-axis Major ..	0·51776	
Mean Daily Motion ..	597″·919	

And the next Passage through Perihelion 1873, May 5·0.

From these elements brought forward to 1873, April 0, the following ephemerides have been computed, the perihelion being assumed, in the first case, to fall on May 5·0, and secondly, four days later, with the view of indicating the line in which the comet should be sought.

The expression for the comet's heliocentric co-ordinates are, (E = the excentric anomaly):—

$$x = [0·47589] \sin (E + 324° 47′·9) + 0·80130$$
$$y = [0·46228] \sin (E + 245 \ 4·1) + 1·22163$$
$$z = [0·15122] \sin (E + 223 \ 57·2) + 0·45684$$

During the interval comprised by the ephemerides, the comet is likely to be very faint. At the date of the last observation by Mr. Schmidt at Athens, in August 1867, the intensity of light was 0·21, which is not greatly exceeded in the next period of absence of moonlight. Nevertheless the comet may perhaps be found with some of our larger instruments, and its early discovery and observation for as long a period as possible appears very desirable, as means may thereby be afforded for an independent determination of the mass of *Jupiter*.

	Perihelion Passage, 1873, May 5·0.				Perihelion Passage, May 9·0.	
^{h} G.M.T.	R.A.	N.P.D.	Log. Δ	I.	R.A.	N.P.D.
	h m s	° ′			h m s	° ′
March 22	16 24 19	102 30·1	0·0567	0·24	16 13 9	101 23·9
23	25 42	35·3			14 25	28·7
24	27 3	40·6	0·0494	0·25	15 39	33·6
25	28 23	45·8			16 52	38·5
26	29 42	51·1	0·0420	0·26	18 3	43·4
27	30 59	102 56·4			19 13	48·3
28	32 15	103 1·8	0·0347	0·27	20 21	53·3
29	33 29	7·1			21 28	101 58·3
30	34 41	12·5	0·0273	0·28	22 33	102 3·4
31	35 52	17·9			23 36	8·5
April 1	37 1	23·3	0·0200	0·29	24 38	13·6
2	38 8	28·7			25 38	18·8
3	39 14	34·2	0·0107	0·30	26 37	24·0
4	40 18	39·7			27 34	29·2
5	41 20	45·3	0·0055	0·31	28 28	34·5
6	42 21	50·9			29 20	39·8
7	16 43 20	103 56·5	9·9984	0·32	16 30 11	102 45·2

Tempel's Comet of Short Period.

Letter from Dr. Bruhns, Director of the Observatory, at Leipsic, to Mr. Hind, dated 1873, March 17.

A pupil of mine, Dr. Seeliger, to facilitate the discovery of the periodical Comet of Tempel, 1867 II, has calculated the perturbations by *Jupiter*. He has started from Mr. Sandberg's elements, and finds,—

	Elements 1867. T 1867, May, 23·958 Berlin.			Elements 1873. 1873, May, 8·95 Berlin.	
$\pi - \Omega$	..	134° 59′ 14″	M. Eq. 1867·0	.. 160° 7′ 24″	M. Eq. 1873·0.
Ω	..	101 10 10		.. 77 58 7	
i	..	6 24 35		.. 9 54 11	
φ	..	30 38 39		.. 28 35 4	
μ	..	623″·044		.. 591″·889	

The time of perihelion in 1873 is retarded fully 117 days by the perturbations. These perturbations are thus large because the comet has approached so near to *Jupiter*: the distances were,—

1867, May 4	..	5·15	1870, Jan. 28	..	0·32
Nov. 20	..	2·93	Aug. 16	..	0·82
1868, June 7	..	1·69	1871, Mar. 4	..	1·29
Dec. 24	..	1·08	Sept. 20	..	1·73
1869, July 12	..	0·65	1872, Apr. 7	..	1·94
			Oct. 24	..	2·65

To have an idea of the comet's intensity of light, it may be noted that on 1867, April 12, log. $r = 0{\cdot}2080$, log. $\Delta = 9{\cdot}8173$, and at the last observation, August 21, log. $r = 0{\cdot}2504$, log. $\Delta = 0{\cdot}0863$.

Ephemeris of Tempel's Comet (1867 II.)

For 12^h Berlin Mean Time.

1873.	R.A. h m s	Decl. ° ′	Log. Δ	Log. r.
Mar. 23	16 16 25	− 9 36·7	0·0252	0·2483
25	18 57	45·5		
27	21 25	54·5		
29	23 46	−10 3·6		
31	26 0	12·8		
Apr. 2	28 8	22·2	9·9870	0·2436
4	30 9	31·3		
6	32 4	41·7		
8	33 50	51·9		
10	35 28	−11 2·5		
12	36 59	13·5	9·9503	0·2400
14	38 23	24·9		
16	39 39	36·7		
18	40 46	49·1		
20	41 44	−12 2·0		
22	42 33	15·5	5·9168	0·2374
24	43 14	29·4		
26	43 47	43·9		
28	44 11	59·2		
30	44 25	−13 15·2		
May 2	44 31	32·1	9·8883	0·2360
4	44 30	49·4		
6	44 21	−14 7·3		
8	44 3	26·0		
10	43 47	45·4		
12	43 4	−15 5·4	9·8674	0·2359
14	42 25	26·1		
16	41 41	47·4		

1873.	R.A. h m s	Decl. ° ′	Log. Δ	Log. r.
May 18	16 40 51	−16 9·1		
20	39 55	31·1		
22	38 55	53·5	9·8567	0·2369
24	37 45	−17 16·4		
26	36 37	39·5		
28	35 29	−18 3·0		
30	34 21	26·8		
June 1	16 33 12	−18 50·8	9·8576	0·2391

For a variation of ± 10 days in time of perihelion passage, the places are:—

	T − 10 Days. h m	° ′		T + 10 Days. h m	° ′
Mar. 23	16 45·2	−12 31	..	15 47·1	− 6 15
Apr. 22	17 21·7	−15 30	..	16 1·0	− 8 14
May 22	17 28·3	−20 18	..	15 49·3	−12 31

Note by Mr. Hind.

Using the log. distances in the above ephemeris, the intensity of light, as represented by $\frac{1}{r^2 \Delta^2}$, will be,—

Mar. 23	..	0·29	May 2	..	0·56
Apr. 2	..	0·35	12	..	0·62
12	..	0·42	22	..	0·65
22	..	0·49	June 1	..	0·64

For comparison with these numbers we have,—

1867, Apr. 3	I = 0·75	Discovered by Tempel. "Faint and diffused."
21	1·06	Easily observed at Leipsic in moonlight.
May 6	1·23	Very bright; nucleus = ✱ 9·7 magnitude (Leipsic.)
29	1·18	Pretty bright; nucleus = ✱ 11·5 ,,
June 22	0·85	Very well seen; nucleus = ✱ 12 ,,
July 7	0·63	Up to this date well seen by Dr. Peters (Clinton, U.S.) "A star-like nucleus admitted of very accurate pointing."
Aug. 21	0·21	Last observed by Schmidt at Athens: 2′ diameter, extremely faint. Glimpsed till August 27.

These values of I are calculated from Mr. Searle's elements. It appears almost certain that large telescopes will be required for the proper observation of the comet at this appearance.

On the Progress to Accuracy of Logarithmic Tables.
By J. W. L. Glaisher, B.A., Fellow of Trinity College, Cambridge.

At the meeting of the Society on June 14 (*Notices*, vol. xxxii. p. 290), in the course of some remarks on the original logarithmic tables, I stated that it was my intention to form at once a complete list of the errors in Vlacq's table of 1628 that would affect a seven-figure table of the logarithms of numbers, with the view of seeing whether any of them were still reproduced in the modern tables. After what has appeared on the subject (*Notices*, vol. xxxii. pp. 255, &c., and pp. 288, &c., May and June, 1872), it is only necessary here to recall to mind that the first table of decimal logarithms published was Briggs's *Logarithmorum Chilias Prima*, London, 1617, containing the logarithms of only the first thousand numbers to 14 decimals. This was followed by Briggs's *Arithmetica Logarithmica*, London, 1624, containing the logarithms of the numbers from 1 to 20,000, and from 70,000 to 90,000 to 14 decimals; and in 1628 Vlacq filled in the gap of 70,000, and published at Gouda his *Arithmetica Logarithmica*, giving the logarithms of the numbers from 1 to 100,000 to 10 decimals. This last work was also published with an English introduction by one George Miller, London, 1631.

Vlacq's work of 1628 being the first that gave the logarithms of the unbroken series of numbers from 1 to 100,000, is the original from which every complete table has been copied, either directly or indirectly, and after more or less revision, for (with the exception of the French manuscript tables that have never been published) no fresh calculation has been made. In vol. xxxii. p. 255, of the *Notices*, I have given references to all the places where, as far as I could find, errors in Vlacq had been published, and certain of these are reprinted there, so that that paper, taken together with Vega's and Lefort's lists, gives very likely all the known errors. As, however, Vlacq's is still the most convenient and accessible ten-figure table, and (with the exception of Vega, 1794) is unique, it would be desirable to collect in one place the errata from every source, and print them all together in one place; but such a list, containing over 600 errata, would occupy so much space, and be used by so few, that the advisability of publishing it, at all events in the *Notices* of this Society, seems questionable. This objection, however, does not apply to the errors which would affect a seven-figure table, and which, though considerably more numerous than I anticipated, amount to only 171, less than one-third of the total number. My object in making the list originally was to see if all the errors were corrected in the seven-figure tables now in use, and if not, to form separate and subsidiary lists for each of the latter, supplemented by any others that I could collect from other sources. This intention was modified for two reasons; first, because it appeared, on comparison, that the best modern tables are quite

free from the Vlacq's errors ; and secondly, because the places in which errors in logarithmic tables have been published are so numerous and so scattered that it would require a considerable amount of research, and occupy much time to treat the subject with only tolerable completeness. The latter fact appeared in the course of the preparation of the 1872 report of the Mathematical Tables Committee of the British Association, in which an attempt was made to give, under each work described, references to the various journals, &c., where errata in it had appeared; but even this limited proposal could not, for several reasons, be carried out satisfactorily. The Committee, however, intend to make the formation and publication of as complete lists as can be obtained of errata in mathematical tables now in use (whether logarithmic or not), a prominent feature in a future report; and this will supersede the partial performance of the same work which I originally intended to give here. The results, therefore, contained in this note are not presented as having more than slight practical importance, their chief claim to notice being the historic interest attaching to the subject. They are to be regarded in the following light:—A table is published containing a certain number of errors (miscalculations, misprints, &c.) ; it is greatly and extensively used in all the sciences, and repeatedly reprinted during two centuries and a half, though never recalculated. The original errors thus become gradually eliminated, and it is a matter of a good deal of interest to watch the progress of accuracy with the lapse of time, and see how long the struggle lasted before truth ultimately gained the complete victory.

It will be seen that there is an interval of more than 200 years between the original publication and the appearance of the first table that is quite free from the hereditary Vlacq's errors, and that even now the triumph of accuracy is not quite perfect, as the seven-figure table last published in England (1871) contains two of the original errors.

The following is a complete list of all the errors in Vlacq's ten-figure table of 1628 that affect the first seven decimals of the logarithms, the seventh figure being increased if the succeeding figures are greater than 500. I formed the table myself by a careful comparison of Vlacq with the errata lists referred to in the paper in the *Notices*, to which reference has been made above, and I examined it carefully after it was formed, so that I believe it to be quite complete and correct. It contains 171 errata, but of these 77 were given by Vlacq himself after the preface to the *Arithmetica Logarithmica*, so that he can only fairly be charged with 94. I have thought it better, however, to include all in the list, so as to make it complete, but have denoted those which Vlacq himself pointed out, by an asterisk. The propriety of including these is strengthened by the fact that Vlacq's errata list was not reproduced in the English copies of 1631—an omission which shows the extreme carelessness with

which that edition was prepared. As a consequence, many of Vlacq's errors, that he corrected himself, are reproduced by John Newton, 1658, who could not have seen the Gouda edition of 1628.

It is to be noted, that though I have spoken throughout of *Vlacq's* Errors, several of them occurring in the portions of the table from 1 to 20,000, and from 90,000 to 100,000, were reproduced from Briggs, 1624.

List of Errata in Vlacq's "Arithmetica Lotharithmica," Gouda, 1628, that affect the first seven decimals of the Logarithms of the numbers from 1 to 100,000.

No.	Error.	Correction.	No.	Error.	Correction.
* 80	9030894	9030900	*7775	3907004	8907004
* 169	2278868	2278867	*7830	2937618	8937618
* 183	2024511	2624511	8172	9123234	9123284
* 238	3765769	3765770	*8556	8322708	9322708
* 580	7634180	7634280	8688	9389193	9389198
* 590	7798520	7708520	*8692	9391147	9391197
*1239	0930712	0930713	*8832	9460541	9460591
*1298	1132746	1132747	*9174	9925587	9625587
*1309	1169336	1169396	*9176	9626524	9626534
*1321	1209023	1209028	9182	9029373	9629373
*1354	1316177	1316187	*9317	9602761	9692761
*1359	1332175	1332195	*9354	9709973	9709974
*1377	1389334	1389339	9429	9644656	9744656
*1626	2111203	2111205	*9480	9798083	9768083
*2167	3358579	3358589	*9482	6769000	9769000
*2434	3863204	3863206	*9972	9987833	9987823
*2534	4038076	4038066	*9973	9988263	9988258
*2544	4055161	4055171	*10058	0025166	0025116
*3329	5223128	5223138	*10061	0026413	0026411
*3499	5439449	5439439	*10096	0041443	0041493
*4599	6626644	6626634	10822	0333075	0343075
*4906	6907295	6907275	*10847	0353006	0353096
*5126	7097736	7097786	*10859	0357808	0357898
*5194	7155029	7155019	*11440	0584269	0584260
*5222	7178339	7178369	*11469	9595256	0595256
*6207	7928811	7928817	*11920	9762763	0762763
*6257	7663662	7963662	11954	5775133	0775133
*6841	8351106	8351196	*11955	0775495	0775496
*6941	8414230	8414220	12218	0970001	0870001
*6957	8424320	8424220	*12328	0908929	0908926
7392	8667620	8687620	*12398	0933566	0933516

No.	Error.	Correction.	No.	Error.	Correction.
*13274	1230016	1230018	41490	6179439	6179434
*14020	1467481	1467480	42344	6267909	6267919
*14527	1621754	1621759	42506	6284505	6284502
*14763	1691749	1691746	44026	6437042	6437092
*14786	1698501	1698507	44656	6498748	6498798
*15843	1998334	1998374	48033	6815367	6815397
*16461	2164592	2164562	48376	6846267	6846300
*17509	2432623	2432613	48764	6880992	6880993
*17773	2497807	2497607	49328	6931935	6930935
*19009	2789543	2789593	49502	6946237	6946227
19087	2087377	2807377	49717	6965059	6965049
19088	2087604	2807604	*49880	6979284	6979264
*19107	2811923	2811925	50479	7031105	7031107
*19113	2813239	2813289	50507	7033506	7033516
*19195	2831861	2831881	50897	7086922	7066922
19315	2758947	2858947	51193	7092108	7092106
20832	3188310	3187310	52943	7238086	7238085
23806	3766894	3766864	53919	7317419	7317418
23999	3401931	3801931	54033	7326541	7326591
24626	3913940	3913939	55692	7457828	7457928
24862	3955381	3955361	56832	7545925	7545929
27164	4339935	4339937	57628	7606336	7606335
*28423	4536299	4536699	57629	7606410	7606411
28758	4587547	4587587	57941	7629856	7629860
29282	4666017	4666007	59502	7745314	7745316
30972	4909683	4909693	59838	7769781	7769771
33071	5194474	5194473	*60400	7810364	7810369
*33800	5280167	5289167	60844	7842173	7842178
33832	5292277	5293277	61163	7864848	7864888
34259	5347757	5347747	61872	7914972	7914942
34728	5406898	5406798	61984	7922786	7922796
*36560	5630082	5630062	61999	7923947	7923847
36935	5674361	5674381	62090	7930215	7930217
37091	5692615	5692685	62267	7942575	7942579
38321	5834568	5834368	62681	7971358	7971359
*38780	5886018	5886078	62759	7976770	7976760
38889	5898298	5898268	63688	8040596	8040576
38962	5906413	5906412	63747	8044597	8044598
39844	6003429	6003629	64125	8070374	8070274
39845	6003638	6003738	64183	8074100	8074200
40403	6064126	6064136	64445	8091992	8091892
*40598	6080546	6085046	64953	8125962	8125992
41018	6129795	6129745	65097	8135510	8135610
41407	6170728	6170738	65217	8143508	8143608

No.	Error.	Correction.	No	Error.	Correction.
65537	8164876	8164866	77662	8902046	8902086
66607	8235299	8235199	*78700	8959748	8959747
66759	8245198	8245098	80212	9042392	9042393
*67050	8263688	8263988	80554	9060831	9060871
67951	8321958	8321959	81674	9120837	9120838
69318	8408450	8408460	86897	9389948	9390048
69579	8424762	8424782	97105	9872418	9872416
73653	8671804	8671904	97828	9904532	9904632
74703	8733480	8733380	98921	9952865	9952885
74742	8735642	8735647	*99090	9960294	9960298
74792	8738557	8738551			

The first thing that strikes any one on looking at this list is the great number of errata that are clearly only misprints—some of them very gross; thus in log. 11469 and log 11920 the first figure is printed a 9 instead of a 0. Such errors as these no amount of carelessness on the part of an editor could reproduce, as they could not occur in seven-figure tables, arranged in the manner now universal. Another point that attracts attention is the great number of errors between 1 and 10,000, all of which must be mere misprints, as they are not reproduced in the logarithms of the corresponding numbers multiplied by 10, that occur in the portion of the table between 10,000 and 100,000. Vlacq's table is so far exceptional that it proceeds from 1 to 100,000, while all its successors, which extend to 100,000 (except Roe, 1633), commence at 10,000, or thereabouts; it is proper, therefore, in making comparisons, to subtract from Vlacq's total of 171 the number of errors occurring in the first ten thousand logarithms (48), and we have 123 left. Errors in Vlacq's differences have not, of course, been noticed. It is, perhaps, worth while to give the errors in log. 80, which is printed 9030893870 instead of 9030899870, as in all the copies but one that I have seen the correction has been carefully made with the pen, so as to render it impossible to see how the logarithm was originally printed.

The following is a list of seven-figure tables (extending from 10,000 to 100,000), arranged in order of date, together with the number of the above-mentioned 123 Vlacq's errors that are reproduced in each. The list has no claim whatever to completeness; it merely is formed of the books satisfying the requisite conditions that I could conveniently lay my hands upon. There will not be found, however, I think, any important omission, as all the leading works which form the main line of descent from Vlacq to the present day are included, and the tables left out consist chiefly of unimportant foreign reprints, which (though the addition of them would have afforded interesting results) were not necessary for the purpose I had in view:—

Vlacq, Arithmetica Logarithmica, Gouda, 1628, fol. (ten decimals), and Logarithmicall Arithmetike, London, 1631, fol. (ten decimals)	123
John Newton,* Trigonometria Britanica, London, 1658, fol. (eight decimals)	98
Sherwin, Mathematical Tables, London, 1706, 8vo,†	65
„ „ 2nd edition, London, 1726, 8vo	58
„ „ 3rd edition „ 1741 „	20
Gardiner, Tables of Logarithms, London, 1742, 4to	19
„ Tables de Logarithmes, Avignon, 1770, 4to (edited by Pezenas, Blanchard, and Dumas)	17
Sherwin, Mathematical Tables, 5th edition, London, 1771, 8vo (edited by Samuel Clark)	20
Hutton, Mathematical Tables, London, 1785, 8vo	12
Taylor, Tables of Logarithms, London, 1792, large 4to	6
Callet, Tables Portatives de Logarithmes, Paris, 1793, 8vo	11
Vega, Thesaurus Logarithmorum completus, Leipzig, 1794, fol. (ten decimals)	23
Hutton, Mathematical Tables, London, 1794, 8vo	10
Vega, Tabulæ Logarithmo-trigonometricæ, edit. secunda, Leipzig, 1797, 2 vols. 8vo	5
Hutton, Mathematical Tables, 3rd edit. London, 1801, 8vo	5
„ „ 6th edit. London, 1822 „	5
Babbage, Table of Logarithms, London. 1827, 8vo	1
Hassler, Tabulæ Logarithmicæ, New York, 1830, 12mo. (also with English, French, &c. titles)	2
Babbage, Tables of Logarithms, London, 1831. 8vo	1
Hülsse, Vega's Sammlung Mathematischer Tafeln, Leipzig, 1840, 8vo	1
Babbage, Table of Logarithms, London, 1841, 8vo	1
Shortrede,† Logarithmic Tables, Edinburgh, 1844, large 8vo	7
Bell, Mathematical Tables (Chambers' Educational Course), Edinburgh, 1847, 8vo	6
Shortrede,‡ Logarithmic Tables, Edinburgh, 1849, 2 vols. large 8vo	6
Anonymous, Mathematical Tables (Chambers' Educational Course), Edinburgh, 1853, 8vo	6
Hutton, Mathematical Tables, London, 1855, 8vo (edited by Olinthus Gregory)	4
Callet, Tables de Logarithmes, Paris, 1855, 8vo	2
Bremiker, Vega's Logarithmic Tables (translated from the fortieth edition of Dr. Bremiker's, by W. L. F. Fischer), Berlin, 1857, 8vo	0
Schrön, Siebenstellige gemeine Logarithmen, Braunschweig, 1860, large 8vo	0
Callet, Tables de Logarithmes, Paris, 1862, 8vo (Revue par J. Dupuis)	0

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* This table gives the logarithms to eight decimals; in making the comparison it was treated as a seven-figure table (last figure corrected), care being taken to give it the benefit of the doubt if the eighth decimal was a 5.

† In this list the terms '8vo.' and '4to.' are used merely to convey an idea of the size of the work, without regard to the mode of printing. Thus Shortrede, Bruhns, and Sang, are nearly exactly the same size, although, as far as the printing is concerned, the first two are 4to. and the last 8vo. A similar remark applies to the editions of Hutton, the earlier ones being 4to. and the more recent 8vo, although the size is the same. Except for purely bibliographical purposes, the description of a book as 8vo, 4to—these words being used in their technical sense—is at all events useless, and may be worse, misleading.

‡ Shortrede commences at 10.800, but this makes no difference in the comparison with other works, as none of the more recent tables contain any Vlacq's errors between 10,000 and 12,000.

Bruhns, a New Manual of Logarithms, Leipzig. 1870, large 8vo 0
Sang, a New Table of Seven-place Logarithms, London, 1871, large 8vo 2

The comparisons were only made for portions of the table, from 10,000 to 100,000. In most works there is a separate table of the logarithms of the first 1000 or 1200 numbers prefixed; but these are, no doubt, free from (hereditary) error.

The above list shows the progress to accuracy of logarithmic tables; of Vlacq's 123 errors he corrected 34 in the errata-list prefixed to his work. Of this list, which did not appear in the English copies of 1631, John Newton was not aware, so that, though apparently correcting 25 of the errors, his table was really retrograde as far as accuracy was concerned, for it contains 98 errors, against 89 only which had escaped Vlacq. I must, however, here state that in counting the errors in any work, I have in all cases *ignored any errata-list*, whether occurring in the work itself or elsewhere. The adoption of this rule (though it presses very unfairly on one or two editors who revised their works after printing, and before publication) was necessary, as a page of errata was often introduced into only the later portion of an impression, or sometimes the errata-list was continually amplified and reset during the making up from time to time of the copies in the same impression. The numbers, therefore, in all cases refer to the number of Vlacq's errors in the table *as printed*, and consequently Vlacq's own number is given as 123. Sherwin, it will be remarked, made improvements in his first and second editions, but the great additional accuracy in the third is due to Gardiner, who carefully revised the work, and published his own table in the succeeding year.

In regard to the next succeeding tables, the most remarkable facts are the near approach to accuracy made by Michael Taylor in 1792, and the great number of errors in Vega, 1794; the latter result is most curious, as Vega is generally regarded (and justly) as the great purifier of the table of logarithms of numbers, and the sole reason why he makes so discreditable an appearance in the above list is the non-recognition of his own errata-lists. (See *Notices*, vol. xxxii. p. 288.) In point of fact every error affecting the first seven places was known to Vega; as in the more complete of his two errata-lists he gives the corrections to *all* the 23 errors that occur in the tabular portion of his work, though, through the omission of several asterisks that ought to have been inserted, a casual reader might imagine that such was not the case. Vega thus in (or soon after) 1794 was acquainted with every error affecting a seven-figure table, and he was the first to be in that position. It is most strange that the editor who detected *every* (seven-place) error should appear in the list as the very worst offender of all. Taylor left uncorrected only six errors, viz. in the logarithms of 38962, 52943, 57628, 57629, 63747, 67951; and of these

the last five are correctly given by Vega in his tables, so that Vega's *tables* (without the errata-list) and Taylor taken together leave only one error, which Vega speedily found out. Thus by about 1794 all the errata were known, and Babbage in 1827 only by a remarkable oversight failed to eliminate them all from his table. The one that occurs is that in log 52943 ;* how this could have escaped detection is most difficult to conceive, as Babbage's table was twice read with the Vega of 1794, in which the logarithm is correctly given. Bremiker's Vega, 1857, is the first work in the list (though, doubtless, not quite the first published), which is free in the table itself from hereditary error. It is well known that Babbage had access for the purpose of comparison to the French manuscript tables, and but for the present investigation, it might be thought that he was enabled to obtain so near an approach to accuracy by their means. Such, however, is in no-wise the case, and seven-figure logarithms owe none of their accuracy to the French tables.

The two errors, which, though discovered by 1794, exhibit such a persistent vitality, are those in log 38962 and log 52943, and they occur in all the tables, subsequent to 1827, where two or more are assigned in the list. It was only so recently as the present year that Mr. Sang wrote to the *Athenæum* (June 8, 1872) about the error in log 52943, which had been pointed out to him as occurring in his table, and which he regarded as a fresh discovery.

It is to be clearly understood that the list shows only the numbers of Vlacq's errors that are reproduced in each of the works named ; and does not convey any information with regard to the total number of errors in any of these works ; it represents, so to speak, the editor's knowledge and research, but not his, or the printer's care in revising the proofs. Thus besides the numbers given there might, as far as the list is concerned, be any number of fresh errata introduced by careless copying and revision, or by false corrections, viz. alterations from right to wrong. Thus the Sherwin of 1771, Hutton speaks of as the most inaccurate table he ever saw, but this is not apparent from the list ; the table is inaccurate, if it be so (and I think it likely that it is) through careless press revision, and a mere cursory examination of the pages shows that not sufficient care has been taken.

With regard to the way in which the tables were compared, I need only say that all the logarithms in the list of Vlacq's errors were examined with each work, and marked according as they were corrected or no. Some I did myself, but the majority were carefully performed by an assistant, who went through the

* A still more curious fact with regard to log 52943 is that although it is correctly given by Vega in his *Thesaurus* (ten decimals) 1794, and in his *Tabulæ* (seven decimals) 1797 ; it was altered from right to wrong by Hülsse in his edition of Vega (seven decimals) 1840, where it appears as of old, before Vega's correction.

work twice: whenever the results appeared to be curious I examined the table myself and always found it correct; the numbers given may therefore, I think, be relied on. In several cases errors were found in the logarithms which were wrong in Vlacq, but in which Vlacq's error was not exactly reproduced. When the error was a misprint, clearly independent of Vlacq, it was not counted, but when it was obviously a partial correction, and had its origin in Vlacq's error it was included. To any one who knows how great has been the number of logarithmic tables that have been published, the list given above may seem meagre, but it is to be remembered that the works included were subjected to a double condition, viz. that they were to give seven decimals and extend to 100,000. This greatly limits the number of works, as very many give only five or six decimals, or extend only from 1000 to 10,000. The complete seven-figure tables form the grand line of succession; and, as I said, the list is fairly perfect; most of the numerous 8vo editions of Vega and Callet are omitted, but they belong to comparatively recent times, and are not of very much importance. The chief succession is Briggs, Vlacq, Roe, Newton, Sherwin, Gardiner, and then there are two branches, viz. Hutton founded on Sherwin, and Callet on Gardiner, and an independent shoot, Vega. I have not included in the list Nathaniel Roe, who published his *Two Tables of Logarithmes* in 1633, and was the first promulgator of a complete seven-figure table, as from his not having corrected the seventh decimal when the succeeding figures were greater than 500, his table cannot properly be compared with the others without a good deal of trouble. The comparison with the Vlacq's errata-list assigns him 3 errors between 1 and 10,000, and 61 between 10,000 and 100,000 (the table extends from 1 to 100,000); but these numbers are not to be relied on at all, even as given a rough estimate of accuracy, as in obtaining them a disagreement was counted as an error or not, according to whether it seemed likely that the discrepancy was due to an error or the want of contraction of the last figure, the benefit of the doubt being nearly always given to Roe. It did not seem worth while making a list of Vlacq's errors to more decimals, for the sake of this one table alone. Roe made the first step towards the arrangement of seven-figure tables as at present, viz. he contracted the numbers by making the last two figures 01,02..50, or 51..99 serve for the whole line, and the logarithms by making the first two or three figures serve for the whole column, and Newton completed the arrangement by making the leading figures of the logarithm common to the block. All things considered, in spite of Vlacq, Vega, Wolfram, and others, it seems to me that this country has throughout fairly kept the lead in logarithmic calculations that it acquired originally by the labours of Napier, Briggs and Gunter.

I have not yet referred to what appears to me to be one of the most interesting points on which the results in this paper throw light, viz. the influence of a perfect 'free trade' in tables. Nothing

could have been left more completely to private enterprise than the publication of logarithmic tables, and the consequence has been that errors have continually reappeared long after their discovery and publication. Had any National Observatory or other Government office, or Academy in any country published tables with their authority, and received and corrected errors as they were pointed out, a perfectly correct table would have no doubt appeared much earlier, and the continual reproduction of inaccuracies would have been avoided. As a matter of fact, the last half-dozen errors that survived were really of no practical consequence, as no one relies on the last figure of a result obtained by logarithmic calculations ; but what the user of a table wants is to be *certain* that it is free from error. As a rule, no one would edit a logarithmic table, unless he considered himself more or less competent to do so ; but the list of errors shows that many who have published such tables were not properly qualified by their knowledge of what had been previously effected with regard to the attainment of accuracy. Take, for example, the case of the late General Shortrede whose reputation as a calculator stands, most deservedly, very high ; his table contains errors not from any want of care in examination, no doubt, but simply because he was not aware of the necessity of making an investigation similar to that contained in this paper, or perhaps was unable (by his residence in India or for some other cause) to do so. The same remark also applies to Mr. Sang and to every one who has published a table containing an error, subsequent to 1794 ; and really also (though the facts in this paper do not render it so apparent) to all the editors of tables previously to that date. Now, had there been any body, such as the Greenwich Observatory or the late Board of Longitude, which would have officially issued a table of logarithms, and undertaken to print every year in their Observations or other periodical publications the errors discovered, besides correcting them in subsequent editions, the amount of accuracy attained, and, what is more important, the amount of confidence felt, in such a work would have been far in excess of what absolutely has been the case. I only advocate a national observatory or other government establishment undertaking such a work, on account of their permanence. A table published by an individual cannot well hold its position for more than forty years for many reasons : Callet and Hutton seem exceptions, but they have been in the hands of great publishing houses which have employed fresh editors from time to time to continually revise them, more or less : they have done, in fact, what I say some government office (or other permanent institution) ought to have done for the last 200 years. Tables, as originally calculated, always *will* have errors which can only be found out one by one in process of time, and I think it is clear that if there is no permanent body which is charged with the reception and corrections of errors as they are found out they will continually be reproduced through the want of knowledge on

the part of editors. Such a superintendence would not in any way infringe on private rights, any more than the publication of the *Nautical Almanac* does. It is to be presumed that if the latter were not published by the State some one would think it worth while to prepare such an almanac, but still no one would regard such a state of things as desirable. Logarithmic tables, besides their continual use in observatories, are of paramount importance in the solution of the triangles in trigonometrical surveys, in fact, Babbage's table was published for use in the calculations connected with the survey of Ireland; so that it is remarkable that it seems never to have occurred to any Astronomer Royal, or Commissioner* of Longitude, that mathematical tables were well worthy the attention of his departments.

All the above remarks apply with even greater force to collections of nautical tables, and the fact that there are not, I believe, any recognised tables published by the Admiralty, and for which it is responsible, is curious. In the course of the preparation of the British Association Report, I examined about twenty collections of nautical tables that have been published during the present century, by naval officers, proprietors of nautical academies, opticians, &c. It is to be inferred that all of these have been more or less used, but I do not see what guarantee of accuracy the purchasers of any could have had; at all events, in the absence of any strong reason to feel confidence in the author, I should not myself like to use the ordinary mathematical tables contained in most of them for calculations. That errors were not very uncommon is evidenced by such phrases as "second edition, with many errors corrected," &c., though, as the publisher's object is to sell his second edition, not to make the first equal in value to the second, there is usually a scrupulous avoidance of any reference as to where the errors occurred. Mrs. Taylor, in the preface to her *Luni-Solar and Horary Tables*, remarks that "some errors have crept into the calculations on account of the multiplicity of entries," for which she claims indulgence on the somewhat singular ground "that the system on which she has worked is mathematically correct and founded on sound principles;" but this naïve confession is, no doubt, pretty well unique, though I dare say the same cannot be said for the necessity thereof. What the collection of tables now in use in the Royal Navy may be I do not know (Raper, I should suppose; Hamilton Moore was the book half a century ago); but be it what it may, I suppose some pains have previously been taken to ensure its accuracy, and if so, it is a pity there is no generally accessible record of such revision having been made. Fresh tables are

* The Board of Longitude seems to have taken no pains to ensure freedom from error or to facilitate the detection or correction of errors, even in works published by themselves as *e.g.* Hutton's *Powers of Numbers*. Errors in logarithms were published in different *Nautical Almanacs*, but there was no systematic attempt made on the part of the Board to render their tables accurate, or even in fact generally useful or known.

continually being published, so that it is clear there is a demand for them; of course, there are different methods of determining the longitude, &c. (and the observational tables need constant improvement), so that one work only would not suffice. All, however, that I wish to point out is, the unsatisfactory nature of the past and present state of affairs with regard to tables for which there is a constant and general demand.* A *permanent* body that would charge itself with the reception and publication of errata in well-known tables would do much; but if such a body would, in addition, itself stereotype and publish the standard tables, continually correcting the plates, so that each edition would become more and more accurate, the errors in the earlier editions being noticed in the introduction, the result would be an amount of accuracy of the tables, and confidence in them on the part of calculators, such as has never yet been experienced, except in isolated cases.† Absolute accuracy is so essential for a table that a conscientious calculator will prefer to perform his computations *ab initio* rather than use a table, of the history, &c. of which he is ignorant; and it seems to me that only by means of some such body as the *Nautical Almanac* office or the Royal Society, is absolute accuracy, and the certainty that such is the case, attainable.

It is, perhaps, too late to do very much for tables of seven-figure logarithms, as it is pretty certain that the later editions of Babbage, Schrön, &c., are free from error, but still it would be something to keep future editors from reproducing obsolete errors; and there are still many tables, viz., ten-figure logarithms of numbers and trigonometrical functions, the natural trigonometrical canon, tables of multiplication, quarter squares, powers, &c., the accuracy of which is very far from being assured. For the next few years the British Association Committee on Mathematical Tables will do what they can with the view of obtaining as much accuracy as possible in tables relating to all branches of mathematics; but the Committee will only exist for a time, and the advantages of a continual supervision will not be in any way obtained.

A list, formed as before, is given below of the Vlacq's errors that occur in several seven-figure tables, the extent of which is generally from 1 to 10,000. Properly Briggs' *Arithmetica*, 1624, is the standard with which such tables ought to be compared; but I have not formed a complete list of errors in Briggs similar

* The high mathematical tables are in one respect better off than the common ones; for, as they cannot but be a pecuniary loss to their authors or editors, no one thinks of publishing one who is not interested in the subject for its own sake, and really earnest in the cause of truth and accuracy: but profit can be made by the sale of the general tables, and they are published *to sell*.

† Barlow's tables (the L. U. K. edition) is a case in point; but it is not every one who would wish to employ the work that is aware of its accuracy. The first logarithmic table stereotyped was, I believe, the 1793 edition of Callet, but I say so only from an impression, not after investigation.

to that given for Vlacq. As this very considerably diminishes the value of the list, I have made no attempt to complete or extend it, merely including, in fact, works I happened to have at hand to give an idea of the general state of care taken in the production of the small tables. Of Vlacq's 48 errors, he only derived 11 from Briggs: this is a confirmation of what was previously remarked, viz., that all the errors in the earlier portion of his table were merely the result of careless copying and revision.

Briggs, Arithmetica logarithmica, London, 1624, fol., 1 to 20,000 and 90,000 to 100,000 (fourteen decimals).	11
Henrion,* Traicté des Logarithmes, Paris, 1626, 8vo (1 to 20,000 fourteen decimals)	8
Brigge† [Briggs], Tables des Logarithmes, Gouda, 1626, 8vo (1 to 10,000)	11
Vlacq, Arithmetica logarithmica, Gouda, 1628, fol. (the portion from 1 to 10,000, ten decimals)	48
Norwood,‡ Trigonometrie, 2nd edition, London, 1651, 8vo (1 to 10,000)	2
Oughtred, Trigonometrie, London 1657, 8vo (1 to 10,000)	1
Gunter,§ Works of Edmund Gunter, 5th edition, 4to. London, 1673, small 4to (1 to 10,000)	3
Vlacq, Tabulæ Sinuum, &c., Amstelædami, 1681, small 8vo (1 to 10,000)	1
Vlacq, Tabellen der Sinuum, &c., Frankfort and Leipzig, 1757, small 8vo (1 to 10,000)	1
Donn, Mathematical Tables, 3rd edition, London, 1789, 8vo (1 to 10,000)	5
Bagay, Nouvelles Tables Astronomiques, Paris, 1829, small 4to (1 to 21,600)	1

All these tables, where it is not otherwise stated, give the logarithms to seven decimals.

De Morgan (in his article on Tables in the *English Cyclopædia*, 1861) makes a suggestion which he thinks may be worth the attention of future compilers, viz. to classify logarithmic tables according as to whether they came from Vlacq or direct from Briggs; and also with regard to their form (number of decimals, &c.), so that they might be divided into sets, those in each set being lineal descendants of their predecessors. I scarcely think it would be possible to carry out this proposal, as some editors (who seem to have imagined all tables must be accurate) have gone back direct to Vlacq and Briggs, while others have merely copied some table with which they happened to be acquainted. A few have also based their tables on a comparison of several. A classification according to sources could only be made by a careful investigation of the errors in each table; but it would scarcely,

* On this work see *Phil. Mag. Supp.*, Dec. 1872 (S. 4. t. xliv. pp. 500 &c.)

† This table is the same as Decker's, Gouda, 1626. See *Phil. Mag.* Oct. 1872.

‡ The tables in this work were also published separately, I believe, under the title "A triangular Canon logarithmicall."

§ The proper date which should be assigned to Gunter's table, is of course, much earlier, before 1630; the date given above merely refers to the particular edition of Gunter's works that I used.

however carefully performed, be of much value or interest. The tables included in the last-written list exhibit one very peculiar feature, which shows how each editor has blindly copied his predecessor, viz. in the log of 9482 which (correcting the seventh figure) is ·9769000 ; but it is printed as ·9768999 in Gunter, Oughtred 1657, Vlacq 1681 and 1757, and Donn 1789 ; although of course it is right in Vlacq 1628, and Briggs 1624, which extend to more decimals. This error, not being one of Vlaeq's, is not counted in the numbers given in the list.

There is a great want of a convenient ten-figure table of the logarithms of numbers, as Vlacq and Vega are both scarce and inaccurate as printed ; and I have some thoughts of reprinting and stereotyping the logarithms of numbers from Vlacq's *Arithmetica*, but whether I do so or not, there is no doubt that, some time or other, it will have to be done, and I therefore take the present opportunity of pointing out the importance of reprinting the table in Vlacq's, and not in Vega's, form. Vega is arranged like a seven-figure table, only as the logarithms having to be given to three figures more than in the latter, the columns headed by the units of the number occupy more than one page, the rest of the double page being occupied with the differences, placed all together : an arrangement exceedingly inconvenient, and liable to cause mistakes. It has the advantage of taking up only about half of the space occupied by Vlacq, but the extent of the latter (600 pp.) is not too great for a working table. I should like to see the portion of Vlacq from 10,000 to 100,000 reprinted exactly as given by him, without any abbreviation whatever, or alteration except that the differences should range with the upper of the two logarithms between which they are placed, that the characteristics should be omitted, catch-numbers and logarithms added at the top of each page, the figures of the *numbers* common to all the column given only at the top perhaps, and the whole printed in such type as to reduce the page to the size of a large octavo.

Mr. Sang's contemplated nine-figure million table will, if carried out, render the use of nine-figure logarithms nearly as convenient as that of seven-figure logarithms is now, except that a table in three volumes is not so handy as one in a single volume ; but even with Mr. Sang's table, ten figures will not unfrequently be required in spite of the additional labour in obtaining the last figure.

While speaking about the arrangement of logarithmic tables, I may be permitted to mention that after examining a great number, it appears to me that Babbage's table, with a few improvements (viz. the separation by a small space of the three leading figures from the other figures of the block, the omission of the subscript points to denote the increase of the last figure, except when that figure is a 5, and the marking the change of the fourth figure of the logarithms in the middle of the line by an asterisk instead of a difference of type) would be the very best possible form. Babbage paid more attention to the mode of printing, the thickness of the

rules, the size of the type,* &c. than any other editor, and in consequence has the most nearly approached perfection. Mr. Sang, in his recently published seven-place table, prints the numbers and logarithms in the same type, and separated by reversed commas, omits all rules, and uses an Arabic nokta (a mark resembling the diamond in a pack of cards) to denote the change in the fourth figure in the middle of the line : all of which he regards as improvements, though after some use of the table, they seem to me to be quite the contrary. The nokta (which was first used by Shortrede in his tables 1844 and 1849) seem very objectionable, and not only the worst of the notations that have been proposed, but worse than no notation at all. It denotes the change well enough when the first figure is a 0, but when it is a 1 or a 2 there is no mark ; and the fact that the latter cases occur but rarely aggravates the evil. A notation that explains its meaning to the eye, without care, ninety-nine times out of a hundred, is more likely to cause error than one that requires care each time.

In a note to my paper in the May number of the *Notices*, I remarked a discrepancy in the dates of the second and third editions of Sherwin, which De Morgan gives as 1717 and 1742, while a copy I had seen bore date 1726. Since writing that note I have taken pains to search out and examine the different editions of Sherwin. The editions I have seen are 1705 (this date being printed in Arabic numerals on the title-page), 1706, Roman numerals, 1717, 1726 ; the third edition, revised by Gardiner, 1741, and 1742 ; the fourth edition 1761, and the fifth edition (by Samuel Clark), 1771, and 1772. It thus appears that it was not at all an uncommon thing (probably as the impression was being made up from time to time) to advance the date by one year. The first four dates we may distribute among the first two editions as we please ; perhaps 1705, 1706, and 1717, are the dates of the first impression, and 1726 of the second. A side-by-side comparison of the books would show the difference between the editions, but I have only seen them separately, and in distant libraries.

With regard to Vlacq's *Arithmetica* referred to in my former communications, I may mention that I have since seen a French edition, Gouda, 1628. (See *Phil. Mag.*, October, 1872.)

I scarcely think it necessary to apologise for making a communication on the subject of logarithms to the Royal Astronomical Society, as, though the method of constructing them is part of the province of mathematics, the tables themselves may well be said to belong to the science in which they find their most useful application. In all the dark ages of astronomy, before it was separated from astrology, its connexion with the trigonometrical canon is one of the facts in its history which it is most pleasant to

* I say nothing about the shape of the figures, a point on which I find it difficult to form a decided opinion.

contemplate : and now that the science has taken its proper rank. I do not think many would wish to see it separated from its old companions, the tables.

Cambridge, Dec. 12, 1872.

Postscript.—In my paper, in the May number of the *Notices*, I expressed an opinion that certain facts "seemed to imply that Vlacq meditated a Dutch translation of his work [the *Arithmetica* of 1628] but that the tables intended for the purpose were bought and published by George Miller." That a Dutch edition was not only proposed, but actually published, is, I think, proved by the following extract from a letter from Briggs to Pell dated October 25, 1628. Speaking of the filling in of the gap in the *Arithmetica* of 1624 he proceeds, "But I am eased of that charge and care by one Adrian Vlacque, an Hollander, who hathe done all the whole 100 chiliades, and printed them in Latin, Dutche, and Frenche, 1000 bookes in these three languages, and hath sould them almost all." The letter is in the Birch MSS., and is printed on p. 55 of the *Letters on Scientific Subjects*, published by the Historical Society of Science, under the editorship of Mr. Halliwell. I intend to quote the whole passage in which the above sentence occurs, in a paper I hope shortly to communicate to the *Philosophical Magazine*, on the subject of Vlacq's and Decker's tables. In all probability, the Dutch copies did not sell, so that they were bought by Miller, and published with the English, in place of the Dutch introduction.

March 14, 1873.

On the Apparent Projection of Stars upon the Moon's Disk in Occultations. By John J. Plummer, Esq.

(*Communicated by Prof. A. S. Farrer, D.D.*)

Few phenomena have more constantly resisted a satisfactory scientific explanation than the well-authenticated though anomalous projection of the images of stars upon the lunar disk previous to occultation. In the *Memoirs of the Royal Astronomical Society*, vol. iii. the whole subject has been thoroughly investigated by Sir James South, and in a later volume (xxviii.) the Astronomer Royal has also given the question careful consideration. The former astronomer, after examining five several hypotheses in order to explain the origin of the phenomenon, is obliged to reject all of them, without assigning any other; and the Astronomer Royal, though more happy, does not appear to consider his solution entirely satisfactory. Indeed, it fails principally in those cases where the phenomenon has been seen at the dark limb, or where a

considerable interval of time, as from 4^s to 9^s, has been observed to elapse before the anomalous appearances have been terminated by the instantaneous disappearance of the star. Yet there can be no doubt, that very many instances of "hanging upon the limb," &c., can be satisfactorily explained by the hypothesis suggested; and we are thus led to the conclusion, that *more than one* explanation must be given to account for all the recorded instances, and further that more than one cause may be in operation at the same time.

If, then, the true explanation of the phenomenon be complicated in this manner, it will be necessary to weed out all the doubtful and fairly explained cases, before we attempt to find another theory to satisfy those instances for which we have no plausible solution; and this is the more necessary, when it is remembered, that the motion of the Moon's limb with reference to the star is never much more rapid than that of the star *Polaris* to the wire of a transit instrument, and may even be slower, should the occultation take place very obliquely to the limb, *i.e.* near the north or south points of the Moon's disk. Practical astronomers in localities where the atmosphere is not good, are only too well acquainted with the unsteadiness of approach of this star, and similar vagaries, if supposed to occur near the Moon's limb, would produce many of the appearances recorded as having been witnessed at occultations.

While engaged some months ago with the consideration of the Moon's libration, it appeared to me possible to frame a theory based on this phenomenon, which should serve as an explanation of the more marked cases of projection; and, as the matter was capable of exact calculation, I determined to submit it to examination at the earliest opportunity. The hypothesis as it presented itself to my mind was as follows. Assuming, as we have a right to do, from M. Hansen's investigations, that the moon's figure is not truly spherical, that the centre of gravity is not coincident with the centre of figure, it follows, that the lunar atmosphere (if any exist) must dispose itself equally on all sides of the point diametrically opposite to the centre of the lunar disk, as seen by us when the libration is at its mean state; and further that the lunar atmosphere may be brought to the limb of the Moon whenever the libration is considerable.

The density of the outskirts of such lunar atmosphere, which is all we can have to do with in this inquiry, would of course be but slight; but should an occultation take place near the point of maximum libration, it may easily be conceived as sufficient to produce a refraction, capable of diverting the stellar ray to the extent which is witnessed. To test this theory all that was required to be done was to compute the amount of the libration effective at the point of the limb at which the occultation took place, for all those cases in which the projection has been noticed in its greatest extent. Unfortunately these cases are too few, especially during the last twenty years, to which period I have at present confined

my attention; but the result will show, that in no case I have examined has the phenomenon been witnessed when there was no libration whatever at the point of occultation.

It is to be remarked that in tracing the origin of the phenomenon to a lunar atmosphere, I am only resuscitating under peculiar circumstances Sir James South's third hypothesis; but it must be remembered that he has rejected it, nor could he have imagined the important part which libration has to play in producing it, the want of symmetry of the Moon's figure being unknown and unsuspected till long after the date of his communication.

At the occultation of ζ *Tauri* on March 28th, 1868, I had the good fortune to witness the phenomenon myself, and the event is indelibly fixed in my memory by the surprising beauty of the spectacle, as well as by the entirely unexpected nature of the occurrence. The disappearance took place at the dark limb which was distinctly visible, and the atmospheric circumstances were eminently favourable. The star remained on the disk for at least five seconds, and very possibly longer, and its distance from the limb was considerable.* Calculating the libration for this observation, I find it amounted to $8^\circ\ 16'$, where it was at a maximum, and $7^\circ 14'$ at the point of disappearance. Both these angles would be slightly increased by the diurnal libration, which I have not taken into account.

On October 14th, 1870, the disappearance of ζ *Tauri*, at the Moon's bright limb, was seen to be accompanied by similar phenomena by Mr. W. H. M. Christie, at the Greenwich Observatory, though the duration of the appearance is not given. On this occasion the Moon's libration was $5^\circ\ 3'$, of which $4^\circ\ 47'$ was effective at the point of occultation. The Moon being only 40^m west of the meridian, the diurnal libration would only *very* slightly increase these angles.

Another Greenwich observer, Mr. Dunkin, has noted the projection at the bright limb, upon the reappearance of α *Geminorum* upon April 4th, 1854, the duration given being 4 seconds. The libration calculated for this observation is $4^\circ\ 6'$, of which $3^\circ\ 43'$ was effective at the point of reappearance. The diurnal libration tends to diminish the angles, but it is noteworthy, that the occultation took place very near the south point of the Moon, and hence at an acute angle with the limb, a circumstance which would tend to give a considerable duration to the phenomenon, even if the star

* This occultation has been reduced by the same method as that in use at the Cambridge Observatory, the *actual* time of disappearance having been taken (not that of contact with the apparent limb), and for the result it has been found that the left hand members of the final equations for the disappearance and the reappearance differ only by $0''{\cdot}7$. The motion of the Moon's limb would have been more than $3''{\cdot}0$ in the interval of projection. Whether the phenomena be regarded as optical or physical, it appears strange that some observers in similar cases have taken the time of apparent contact with the limb as that of the true disappearance.

had only been projected a short distance within the limb. Mr. Dunkin's note would seem to bear out this remark.

A fourth instance is recorded by Mr. Talmage at Leyton, on March 19th, 1866, at the disappearance of 31 *Arietis* at the dark limb. The libration has been found to amount to 3° 27′, of which 2° 52′ was effective at the point of occultation, which has been assumed from the *Nautical Almanac* as the same as for Greenwich. The diurnal libration would increase these angles, making them about equal to those of the previous example, similarly corrected. Neither of these two last instances is sufficiently strong to support the hypothesis alone, though they certainly do not contradict it ; and it is always open to suppose the occultation to have taken place in a valley or depression, which would be equivalent to a sensibly augmented libration.

These are the only instances I have been able to discover during the past twenty years, but there is one case recorded in the *Greenwich Observations*, which is deserving of some consideration from the circumstances, that *two* observers noted the hanging upon the limb for five seconds after the reappearance of the star. This happened at the occultation of *Regulus* upon May 19th, 1858, Calculating as before I find the total libration to amount to 7° 10′, and at the point of reappearance to 6° 58′. The diurnal libration slightly decreases the angles.

The cases adduced may not be sufficient to warrant the conclusion that a lunar atmosphere exists and is the cause of this troublesome and anomalous peculiarity; but it is hoped they are sufficient to show, that there is strong *primâ facie* evidence in favour of the supposition, more than enough to render the careful observation of occultations highly desirable. It may be urged that the theory proves too much, and that since the Moon is so rarely near the state of mean libration, the phenomenon should occur at either the disappearance or reappearance of many occulted stars. It may be replied, that its occurrence at the dark limb must always be invisible, unless that limb be well illuminated by earth-light, which happens but seldom, and that at the bright limb, especially if a reappearance, the observation is one of such acknowledged difficulty, that an observer is both fortunate and sharp-sighted to detect it. Moreover, the majority of observed occultations are those happening before the full moon, and of the comparatively few observed after, it is only in those of bright stars that the phenomena can fairly be expected to be seen.

There is, however, a possibility that the existence of a lunar atmosphere might be established in an entirely independent manner by the spectroscope, and I am unaware that libration has yet been taken advantage of for this purpose. The observation would be a delicate one, as only very slight density of atmosphere is possible. The Durham Observatory is supplied with no more powerful instrument than a Browning star-spectroscope, furnished with two 30° prisms, and is therefore manifestly ill suited for undertaking

such an inquiry. I have attempted the observation with this instrument, it is true, but I need scarcely add, unsuccessfully.

Durham Observatory,
Feb. 22, 1873.

P.S.—In the *Memoirs of the Royal Astronomical Society*, vol. xxviii. pp. 117, *et seq.* there are recorded no less than six cases of projection seen by Captain Jacob at the Madras Observatory in the short space of sixteen months, all of which I have steadily ignored. They are all noted as distinct cases of projection, but no estimate of the duration or the distance from the limb is given. Respecting two of them, however, a note may be found in the *Monthly Notices*, vol. xvii. page 17, written only eight days after the observations were made, in which that meritorious observer expresses himself far more doubtfully, and indeed appears to describe elaborately all the peculiarities of a case of "hanging upon the limb." This leads me to question, whether Captain Jacob ever saw a star fully projected on the disk, and to reject his observations as doubtful. I am unaware whether the Astronomer Royal, who places these observations in the list of certain projections, had any further evidence to guide him in this matter.

Note on Mr. Plummer's Paper on the Apparent Projection of Stars. By Richard A. Proctor, B.A., Cambridge.

It is commonly assumed that if the Moon's figure be such as Hansen supposes (though it must be remembered that Newcomb disputes Hansen's conclusion), the atmosphere would be on the further hemisphere of the Moon, and disposed symmetrically around the point antipodal to the centre of the visible disk at the time of mean libration. But I conceive that a very different arrangement would take place,—the densest part of the lunar atmosphere lying round the parts of the Moon which form the outline of her disk at the time of mean libration. Let us assume, with Hansen, that the Moon's surface is formed of two spherical surfaces, the part nearest to us having the least radius, so that in fact the Moon is shaped like a sphere to which a meniscus is added, said meniscus lying on the visible hemisphere. If we imagine the meniscus removed, the lunar atmosphere would dispose itself symmetrically round the Moon's spherical surface. Now, suppose that while this state of things exists, the lunar air within the region now occupied by the meniscus of solid matter is suddenly changed to matter of the Moon's mean density, what could be the effect of this change, by which new matter would

be added on the side of the Moon towards the Earth? Surely not that the remaining atmosphere would tend to the further side of the Moon, but, on the contrary, that it would be attracted towards the nearer side by the new matter there added. The lunar air would be shallower on this nearer side, no doubt, because the air thus drawn to it would not make up for the air supposed to be changed into the solid form; but at the parts which form the edge of the disk there would be an access of air, without this diminishing cause, and the air would therefore be denser there than elsewhere. But in this final state of things there would be equilibrium; we learn then what are the conditions of equilibrium for a lunar atmosphere, assuming the Moon's globe to have the figure supposed by Hansen. There would be a shallow region in the middle of the visible disk, and a region slightly shallow directly opposite, while the mid-zone would have the deepest atmosphere. But it is around this zone precisely that no signs of a lunar atmosphere have as yet been recognised.

I may remark that this reasoning may be extended to the Earth. Assuming the waters of the Earth drawn towards the South Pole because of a displacement in the Earth's centre of gravity, we may regard the surface of the sea in the southern hemisphere as standing above the mean surface of the globe, and a part of the southern seas as therefore constituting a meniscus like that conceived by Hansen to exist in the case of the Moon. It would follow, then, if my reasoning be correct, that we should have the atmosphere shallowest in light southern latitudes—shallow, but only slightly so, in high northern latitudes, and densest between the tropics; but this, as is well known, is precisely the observed arrangement.*

* The shallowness of the air in high Southern latitudes was one of the most remarkable phenomena discovered during Ross's Antarctic voyages; and at the risk of having Antarctic voyaging regarded as being with me like King Charles's head in Mr. Dick's memorial, I venture to note that it would be most desirable to obtain more information as to this remarkable feature of the atmosphere.

Catalogue of 81 *Double Stars, discovered with a* 6*-inch Alvan Clark Refractor.* By S. W. Burnham, Chicago, U.S.A.

It is believed that the stars enumerated in the accompanying list have been hitherto unknown as double stars, as they are not found noted in the numerous catalogues and publications relating to this subject. With a single exception they were discovered with a 6-inch Alvan Clark refractor, and but three or four of them have been seen by me with any other instrument. The position angles and distances are estimated, except in the few instances where measures have been made by George Knott, Esq., as stated in the "Notes." The magnitude of the primary is generally that given in the star-catalogue from which its place is taken, the secondary being rated according to Herschel's scale.

The more prominent works which have been examined are:— Struve's *Mensuræ Micrometricæ;* Otto Struve's *Catalogue de* 514 *Etoiles Doubles et Multiples*, &c., 1843, and the revised edition of 1850; Herschel and South's *Catalogue of* 380 *Double and Triple Stars* ("Philosophical Transactions," 1824); South's *Catalogue of* 458 *Double and Triple Stars* ("Philosophical Transactions," 1826); Secchi's *Catalogo di* 1321 *Stelle Doppie*, &c. Herschel's *Results of Astronomical Observations at the Cape of Good Hope;* the seven series of Sir John Herschel in the *Memoirs of the Royal Astronomical Society; First Catalogue of* 321 *New Double and Triple Stars* (Vol. II.); *Second Catalogue of* 295 *New Double and Triple Stars* (Vol. III.); *Third Catalogue of* 384 *New Double and Triple Stars* (Vol. III.); *Fourth Catalogue of* 1236 *New Double and Triple Stars* (Vol. IV.); *Fifth Catalogue of* 1304 *New Double and Triple Stars* (Vol. VI.); *Sixth Catalogue of* 286 *New Double and Triple Stars* (Vol. IX.); *Seventh Catalogue of* 84 *New Double and Triple Stars* (Vol. XXXVIII.); the several Catalogues of *Jacob, Dunlop, Wrottesley, Powell, Smyth, Fletcher, Dawes*, and others, found in the *Memoirs of the Royal Astronomical Society;* and the discoveries of Dawes, Clark, Dembowski, Winnecke, Knott, Otto Struve, and others, in *Monthly Notices, Astronomische Nachrichten*, &c., embracing altogether nearly 8000 different double stars visible in this latitude.

No.	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
1	Arg. (+55°) 191 A and B	0 45 12	+55 55	90	1·5	7¾, 11	Sept. 12, 1871, and Sept. 14, 1872	One of the finest multiple stars in the heavens. B and E very difficult, and at first overlooked. Numerous other stars in the field more distant. $12^m\ 4^s$ *f a Cassiopeiæ* and 5′ 43″ north.
	A and C	..	..	135	4	8½		
	A and D	..	..	200	10	10		
	A and E	..	..	360	15	15		
2	Weisse i. 16	1 3 12	+29 11	160	1·5	9, 10½	Sept. 26, 1872	$1^m\ 16^s$ *p τ* * *Piscium*, and 13′ 19″ south.
3	Arg. (+55°) 278	1 9 3	+55 49	40	5	8, 11	Sept. 6, 1871	In *Cassiopeia*.
4	Weisse i. 236	1 16 2	+10 45	100	0·5	7, 7½	Oct. 24, 1872	Very close and difficult pair in *Pisces*. The brightest of two stars about 12′ apart; the other *s p*.
5	103 Piscium	1 32 15	+15 58	300	1·5	7, 12	Nov. 3, 1872	Fine and not very difficult pair; in the same field *n p Piscium*, 6 mag.
6	L. 3205	1 38 13	− 7 25	190	3	7, 10	Nov. 17, 1871	In *Cetus*, *n f* Σ 150.
7	58 Ceti = B.A.C. 598	1 51 23	− 2 42	15	3·5	6½, 14	Nov. 11, 1871	A very delicate and difficult object, resembling 94 *Ceti*, but closer. "Piazzi says he could not find this star. It is probably variable."—*Note* to B.A.C.
8	Arg. (+8°) 364	2 14 27	+ 8 17	200	0·9	8, 10	Nov. 11, 1871	A close and moderately difficult pair, between ξ^1 and ξ^2 *Ceti*.
9	L. 5107 = Weisse ii 921–2	2 39 4	+35 1	165	1·5	7, 9	Oct. 15, 1872	A fine double, $4^m\ 26^s$ *p* 17 *Persei*, and 29′ north.
10	L. 5276 = Weisse ii. 738	2 43 51	− 5 31	100	3	7, 14	Jan. 8, 1872	A somewhat difficult pair, the *f* of three stars forming a triangle in the finder.
11	ϱ^2 Eridani	2 56 19	− 8 12	85	3	5½, 10	Nov. 11, 1871	The companion of this very beautiful pair seems to be variable. I have seen it well with the aperture contracted to 3 in.; at other times, in equally good weather apparently, it required careful attention with the full aperture.

* In MS. r.—Ed.

No.	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
12	Weisse iii. 308	3 18 8	−14 27	270	2·5	7, 10	Nov. 28, 1872	In *Eridanus;* not difficult.
13	Weisse v. 678	5 28 6	− 4 35	130	1	8, 12	Nov. 11, 1871	This excessively difficult pair is in the same low power with Σ 743, 5′43″ *s*, a little *p*, and is No. 381 of Bond's "Catalogue of Stars about the great Nebula of *Orion*."
14	L. 10696	5 34 10	+29 47	200	5	8, 11	Mar. 19, 1872	In *Auriga*, 30′ *n f* Σ 764.
15	Weisse v. 1022	5 41 14	− 2 21	190	2	8, 14	Feb. 2, 1872	Another very difficult pair in *Orion*, 8ᵐ 2ˢ *f* ζ *Orionis*, and 20′ *s*.
16	3 Monocerotis	5 55 43	−10 36	356·1	1·79	5½, 10	Dec. 20, 1871	The measures of distance and position of this fine pair were made by George Knott, Esq.
17	4 Monocerotis	6 2 19	−11 7	178·6	3·38	6, 10½	Mar. 3, 1871	Measured by George Knott, Esq., who has detected a 11½ mag. companion at 244·1°; D = 10″.
18	L. 12006	6 10 39	−12 0	280	1·5	7½, 10	Dec. 27, 1871	A close but easy pair *p* Σ 3116. It is singular that Struve should have missed this and the two preceding objects in the same vicinity, as they are comparatively easy.
19	L. 12936	6 36 8	−15 53	169·4	4·07	7, 9½	Feb. 22, 1871	A very easy pair in *Canis Major*, *n p Sirius*. Measured by George Knott, Esq.
20	L. 13170	6 42 58	−16 4	30	3	8, 12	Jan. 8, 1872	The middle of three stars nearly in a line, *n f Sirius*. The companion is very faint and rather difficult.
21	*n* Canis Minoris	7 21 2	+ 7 12	30	5	6, 14	Feb. 2, 1872	The companion an exceedingly minute point of light.
22	Weisse vii. 689	7 24 51	+33 7	155	7	8½, 11½	Feb. 11, 1872	In *Gemini*, nearly 1° *n.* of *Castor*.
23	Anonyma	7 55 44	+ 3 26	180	3	8, 13	Feb. 2, 1872	A difficult double in *Canis Minoris*, *n f* a 7-mag. star. There is a very faint pair in the *n p* quadrant, about 100″ from the primary.
24	L. 17586	8 47 55	− 8 16	170	1·25	7½, 9½	April 18, 1872	This close pair is about ¾° *s.* of 17 *Hydræ* (Σ 1295).

No.	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
25	Weisse x. 242	10 15 15	− 9 7	190°	1″75	8, 10	April 8, 1872	In *Sextans*.
26	L. 21697	11 17 12	− 9 42	75	2	7, 10½	April 3, 1872	This very pretty pair is 30′ from *s Crateris*, *n. p.*
27	L. 23106	12 13 29	+14 35	110	4	7, 12	April 22, 1872	An interesting double star, about 1½° *s. f.* 6 *Comæ*. This star is Weisse xii. 197.
28	B.A.C. 4213	12 23 23	−12 40	355	3	6½, 12	April 10, 1872	Very delicate and beautiful double star, preceding and nearly equidistant from *Corvi*, 45 B and 48 B. This star is P. xii. 104.
29	L. 23536	12 28 57	−16 7	260	10	7, 13	May 3, 1872	A delicate object; in *Corvus*, $5^m\,45^s$ *f* ∂ and 20′ *s*.
30	Arg. (+20°) 2904	13 51 57	+20 5	220	5	8½, 14	May 24, 1872	In *Boötes*, $3^m\,50^s$ *p* Σ 1797.
31	L 27106	14 46 31	+19 16	180	1	8, 10	May 4, 1872	This very beautiful and close pair is about 30′ from ξ *Boötis*, *s f;* a difficult object. I have since been informed by Mr. Alvan Clark that he discovered this pair a year or two earlier, but the fact was never published.
32	6 Serpentis	15 14 25	+ 1 11	30	3	5½, 13	June 18, 1872	Splendid double star, but excessively difficult.
33	Weisse xv. 424 = L. 28246	15 24 5	−12 33	45	3	8, 11	June 9, 1872	Two double stars in the same field; the last 4′ from the other, *s f*, in *Libra*.
34	Anonyma	..	..	55	3	10, 10	June 9, 1872	
35	B.A.C. 5184	15 35 27	−15 36	90	2	7, 9	June 11, 1871	This very easy pair is *s p n Libræ*. It is identical with P. xv. 150.
36	2 Scorpii	15 45 48	−24 56	270	3	5½, 10	June 20, 1871	An extremely beautiful object (= B.A.C. 5250).
37	Anonyma	15 54 39	−24 13	40	2	9, 10	June 15, 1872	Faint but difficult pair, 24′ *n p* a 7-mag. star (B.A.C. 5317).
38	L. 29136	15 55 3	−24 39	350	4	7, 11	June 19, 1870	Some distance *n f π Scorpii*.
39	11 Scorpii	16 0 23	−12 24	258·2	3·75	6, 12	June 23, 1872	Strangely missed heretofore. Measured by G. Knott, Esq.

No	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
40	Arg. Z. 388, No. 32	16 3 51	−27 13	350	4	8, 10½	April 27, 1870	52′ north of 12 *Scorpii*, a little *p*.
41	Arg. (+61°) 1583	16 17 16	+61 45	60	1·5	9, 11	June 9, 1871	A difficult pair, 5ᵐ *p n Draconis*, and in the same low-power field with Σ 2045, *s p*.
42	Weisse xvi. 1076	16 34 56	+29 16	35	4	9, 10	May 21, 1872	Faint pair in *Hercules*.
43	Weisse xvi. 785	16 41 48	+ 2 58	70	1	8, 9	June 23, 1872	Close pair 40′ *n* of and a little *p* 19 *Ophiuchi* (Σ 2096).
44	Arg. (+28°) 2697	17 9 11	+28 59	15	7	8½, 11	July 11, 1871	In *Hercules*, 3ᵐ 40ˢ *p* Σ 2147. A faint companion *n f*. D = 25″.
45	Weisse xvii. 345	17 13 6	+32 37	290	5	9, 9½	May 3, 1870	A faint pair 2ᵐ 40ˢ *p* 72 *Herculis*.
46	Weisse xvii. 296	17 17 39	+13 31	190	3	8, 14	June 15, 1872	Difficult; the *p* star of a small equilateral triangle, the *f* star of which is Σ 2159.
47	L. 32978	17 54 19	−10 14	270	1·25	8½, 11½	Aug. 15, 1871	An excessively difficult pair, about 45′ *s f* ν *Ophiuchi*.
48	L. 33729	18 13 19	−19 43	10	2·75	8 11	July 21, 1871	Two faint companions *p*.
49	Anonyma	18 16 28	−19 39	40	7	8, 11½	Aug. 2, 1871	About 1° *n* of 21 *Sagittarii*.
50	Arg. (+39°) 3475 A and B C	18 33 38	+39 29	330	70	8½, 12	June 3, 1872	Excessively difficult double companion; 6ᵐ 15ˢ *p* ε *Lyræ*, in the same field with a 6-mag. star (B.A.C. 6357), 4′·7 exactly *s*.
	B and C	..	..	180	15	12		
51	Arg. (+39°) 3523 A and B C	18 41 21	+39 32	190	90	8½	July 3, 1870	This star with a double companion is 1ᵐ 20ˢ *f* ε¹ (4) *Lyræ*.
	B and C	..	..	280	2	11, 11		
52	Weisse xviii. 1793 A and B	18 58 7	+25 52	300	40	8, 11	Oct. 6, 1872	In the same field with Σ 2444, 23ˢ *p*. The third star exceedingly minute (=L 35688).
	B and C	..	..	165	3	15		
53	Arg. (+11°) 3902	19 29 19	+11 9	255	1·7	8¾, 11	Sept. 14, 1872	In *Aquila*; strangely difficult for magnitudes and distance.

No.	Designation.	R.A. 1870. h m s	Decl. 1870 ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
54	Σ. 2557 A and C	19 34 25	+29 27	290	15	8, 14	Oct. 6, 1872	Weisse, xix. 1088. A minute companion overlooked by Struve.
55	∗'s of γ Aquilæ	19 40 3	+10 15	40	3	10, 10	June 26, 1870	Very faint and difficult pair, about 3′ *s* of *γ Aquilæ*; triple with a third star *p*.
56	Weisse xix. 443	19 58 15	− 4 41	180	2	7½, 10	Aug. 21, 1871	Beautiful pair; the *p* one of three stars forming an equilateral triangle in the finder (=L 38343).
57	L. 38415	19 59 27	+15 8	140	2	7, 15	July 30, 1872	A beautiful, but excessively difficult double; 40′ from 14 *Sayittæ s f*.
58	Weisse xix, 2025	20 1 23	+15 42	190	10	8, 13	July 30, 1872	3ᵐ 50ˢ *f* 14 *Sayittæ*, and 1′ 54″ *n*.
59	Weisse xx. 213	20 10 7	+ 4 44	125	6	8, 11	Oct. 3, 1872	Not very difficult or interesting.
60	π Capricorni	20 19 52	−18 38	144·9	3·15	5, 10	Aug. 8, 1871	The distance and position of this elegant double star measured by George Knott, Esq.
61	ϱ Capricorni A and C	20 21 27	−18 14	135	30	5, 15	Aug. 8, 1871	An excessively difficult companion near the line joining ϱ and the 7½ mag. star *s f*.
62	L. 39445	20 22 42	+29 42	140	1·25	8, 10	Sept. 21, 1871	Close and difficult pair, 25′ *s p* 41 *Cygni*; two distant companions at 180° and 270°.
63	1 Delphini	20 24 4	+10 28	340	0·7	6, 11	Oct. 4, 1872	One of the most difficult of double stars.
64	Weisse xx. 977	20 38 50	+12 15	350	0·8	9, 9½	Aug. 27, 1872	The north star of a wide equal pair about 90″ apart. Discovered with Prof. Young's 9·4-in. refractor.
65	13 Delphini	20 41 22	+ 5 32	195	1·25	6, 10	Nov. 11, 1871	An elegant pair. The first of five double stars found to-night.
66	Arg. (+26°) 3995	20 42 33	+26 58	160	2	8, 8	Oct. 6, 1872	Fine pair in *Vulpecula*.
67	Weisse xx. 1460	20 45 12	+30 26	290	1·25	7, 11½	Sept. 21, 1871	A beautiful but difficult double, 4ᵐ 55ˢ *f* 52 *Cygni*, and 1½° *n* of O Σ 415. This is L. 40318.
68	Arg. (+49°) 3431	20 55 17	+49 43	170	1·5	8½, 9	Sept. 19, 1872	In *Cygnus*, 1ᵐ 56ˢ *f* P. xx. 429 (Σ 2741) and 15′·4 south.

No.	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered,	Notes.
69	Weisse xx. 1743-4	20 56 43	+21 7	350	2	8, 9	June 23, 1872	The *f* star of a wide pair of 8 mag. stars. In *Vulpecula*.
70	L. 40824 A and B C	20 58 23	+11 31	235	70	8	Aug. 5, 1871	In *Delphinus*; of the same class as Nos. 50 and 51.
	B and C	..	..	110	2	10, 10		
71	γ Equulei A and C	21 4 1	+ 9 37	10	25	5, 16	Dec. 2, 1871	Excessively faint. There is a faint triple $1^m 57^s$ *p*. Distances = 3″ and 15″.
72	Weisse xxi. 511	21 23 1	— 5 58	50	2	9, 12	Oct. 7, 1872	Beautiful but very difficult pair, $1^m 31^s$ *p* β *Aquarii* and 10′ 44″ north.
73	β Aquarii A and C	21 24 42	— 6 8	180	35	3, 16	Aug. 8, 1871	Very faint and difficult; fully one magnitude smaller than Herschel's companion.
74	L. 42052	21 29 12	+20 49	315	1·5	6½, 10	Aug. 12, 1871	Fine and moderately difficult pair in *Pegasus*.
75	Weisse xxi. 1133 = L. 42736	21 49 10	+10 16	30	1·25	8, 9	Oct. 1, 1872	Fine pair in *Pegasus*.
76	Weisse xxii 465 = L. 43906	22 22 58	— 0 52	335	1·5	8½, 12	Oct. 6, 1872	Elegant pair in a low-power field with ζ *Aquarii* 44^s *f*, and 11′ south.
77	Anonyma A and B	22 27 21	— 2 27	210	2	8, 10	Sept. 4, 1872	In the same field with 60 *Aquarii*, 12′·5 exactly south.
	A and C	..	..	225	20	12		
78	Weisse xxii. 1393 A and B	23 1 40	+30 46	80	10	8, 12	Oct. 19, 1872	In *Pegasus*; not difficult.
	A and C	..	..	90	20	14		
79	Weisse xxiii. 187	23 10 54	— 2 14	120	1	8, 10	Oct. 25, 1872	A beautiful but very difficult pair in the same field with Σ 2995, 61^s *f* and 4′ 6″ *n*. (7 mag. in Weisse.)
80	Weisse xxiii. 229	23 12 12	+ 4 42	300	1	8½, 9	Sept. 6, 1872	A fine double star, $1^m 30^s$ *p* 7 *Piscium* 1′ 39″ *s*.
81	Weisse xxiii. 562	23 28 28	—12 18	20	1·5	8, 12	Oct. 23, 1872	Very difficult; in *Aquarius*.

Comparison of the R.A. and N.P.D. of Standard Stars observed at the Radcliffe Observatory, Oxford, in the year 1869, *with the R.A. and N.P.D. founded on the Tabulæ Reductionum.* By Prof. J. Ph. Wolfers.

(*Communicated by the Radcliffe Observer.*)

Names	R.A. 1869. No. of Obs.	Oxford. h m s	W.	O−W. s	Decl. 1869. No. of Obs.	Oxford. ° ′ ″	W.	O−W. ″
α Androm.	4	0 1 37·18	37·20	−0·02	11	+28 22 1·51	2·51	−1·00
γ Pegasi	1	0 6 29·50	29·60	−0·10	9	+14 27 18·95	18·85	+0·10
α Cassiop.	4	0 33 5·16	5·28	−0·12	19	+55 49 6·90	6·75	+0·15
[β Ceti]	1	0 37 0·72	0·75	−0·03	5	−18 42 21·88	22·96	+0·08
α Arietis	1	1 59 47·69	47·62	+0·07	2	+22 50 29·43	30·45	−1·02
[γ Ceti]	4	2 36 30·86	30·93	−0·07	1	+ 2 40 55·17	55·68	−0·51
α Ceti	3	2 55 25·93	26·02	−0·09	4	+ 3 34 27·05	26·31	+0·74
[δ Arietis]	1	3 4 8·54	8·60	−0·06	2	+19 13 46·29	45·50	+0·79
α Persei	..	..	..	..	6	+49 23 32·73	31·98	+0·75
α Tauri	3	4 28 24·32	24·38	−0·06	4	+16 14 36·26	37·30	−1·04
α Aurigæ	2	5 7 0·91	0·96	−0·05	9	+45 51 40·28	41·24	−0·96
β Orionis	2	5 8 14·54	14·63	−0·09	3	− 8 21 19·18	19·06	−0·12
β Tauri	2	5 18 0·68	0·76	−0·08	4	+28 29 37·76	37·28	+0·48
α Orionis	2	5 48 4·93	4·87	+0·06	2	+ 7 22 48·66	47·81	+0·85
α Canis Maj.	16	6 39 22·34	22·39	−0·05	15	−16 32 19·41	19·04	+0·37*
α Geminor.	10	7 26 14·30	14·01	+0·29	18	+32 10 22·50	22·64	−0·14
α Canis Min.	13	7 32 26·59	26·60	−0·01	14	+ 5 33 29·86	30·15	−0·29
β Geminor.	12	7 37 17·83	17·83	0·00	20	+28 20 24·39	24·21	+0·18
[ι Ursæ Maj.]	3	8 50 13·67	13·61	+0·06	5	+48 33 13·68	13·15	+0·53
α Hydræ	5	9 21 8·98	9·02	−0·04	4	− 8 5 30·84	31·30	+0·46
[δ Ursæ Maj.]	1	9 24 5·06	4·78	+0·28	3	+52 16 20·87	21·01	−0·14
α Leonis	11	10 1 23·61	23·60	+0·01	16	+12 36 24·80	23·22	+1·58
[γ¹ Leonis]	9	10 12 44·85	44·81	+0·04	10	+20 30 11·47	11·45	+0·02
α Ursæ Maj.	11	10 55 37·19	37·19	0·00	28	+62 27 27·92	26·55	+1·37
[χ Leonis]	4	10 58 15·38	15·60	−0·22	4	+ 8 2 37·76	36·35	+1·41
[δ Leonis]	6	11 7 8·32	8·42	−0·10	10	+21 14 27·81	28·12	−0·31
[δ Crateris]	3	11 12 47·57	47·63	−0·06	3	−14 4 12·25	12·25	0·00
β Leonis	4	11 42 22·66	22·59	+0·07	9	+15 18 16·16	15·99	+0·17
γ Ursæ Maj.	13	11 46 55·65	55·81	−0·16	19	+54 25 22·42	22·99	−0·57
12^2 Canum Venat.	4	12 49 53·87	53·83	+0·04	8	+39 1 34·47	35·31	−0·84

* MS. indistinct.—Ed.

Names.	No. of Obs.	R.A. 1869. Oxford.	W.	O—W.	No. of Obs.	Decl. 1869. Oxford.	W.	O—W.
		h m s	s	s		° ′ ″	″	″
α Virginis	2	13 18 17·59	17·68	−0·09	2	−10 28 36·02	35·77	−0·25
[ζ Virginis]	2	13 28 1·18	1·31	−0·13	8	+ 0 4 29·56	31·23	−1·67
η Ursæ Maj.	8	13 42 22·56	22·61	−0·05	27	+49 58 4·53	4·78	−0·25
[η Bootis]	9	13 48 26·85	26·96	−0·11	14	+19 3 19·64	20·14	−0·50
α Bootis	18	14 9 41·23	41·22	+0·01	36	+19 51 56·60	57·31	−0·71
1 α (8) Libræ	3	14 43 26·71	26·66	+0·05	3	−15 27 2·89	2·15	−0·74
2 α (α) Libræ	3	14 43 38·11	38·11	0·00	3	−15 29 44·23	43·51	−0·72
β Ursæ Min.	12	14 51 6·76	6·92	−0·16	18	+74 41 27·61	25·42	+2·19
α Coronæ	10	15 29 8·52	8·56	−0·04	20	+27 9 25·35	27·13	−1·78
α Serpentis	3	15 31 49·03	49·03	0·00	3	+ 6 50 21·91	23·56	−1·65
[ζ Ursæ Min.]	5	15 48 47·50	47·84	−0·34	8	+78 11 45·46	46·32	−0·86
α Scorpii	8	16 21 22·74	22·73	+0·01	9	−26 8 18·41	18·06	−0·35
[ξ Herculis]	5	16 36 20·87	21·09	−0·22	12	+31 50 29·43	29·61	+0·18
[κ Ophiuchi]	4	16 51 27·97	28·07	−0·10	5	+ 9 34 50·82	52·21	−1·39
α Herculis	8	17 8 40·51	40·55	−0·04	11	+14 32 30·86	31·15	−0·29
[β Draconis]	3	17 27 28·30	28·43	−0·13	9	+52 23 58·47	57·80	+0·67
α Ophiuchi	6	17 28 51·22	51·26	−0·04	20	+12 39 27·59	28·32	−0·73
[μ Herculis	8	17 41 19·96	19·91	+0·05	9	+27 47 57·01	58·59	−1·58
γ Draconis	4	17 53 33·67	34·01	−0·34	14	+51 30 19·32	19·00	+0·32
α Lyræ	18	18 32 30·14	30·22	−0·08	48	+38 39 47·92	48·21	−0·29
β¹ Lyræ	..	..	..	..	12	+33 12 44·06	44·04	+0·02
[δ Aquilæ]	7	19 18 53·56	53·56	0·00	6	+ 2 51 21·93	21·56	+0·37
γ Aquilæ	2	19 40 1·89	1·93	−0·04	12	+10 17 46·12	46·03	+0·09
α Aquilæ	5	19 44 23·46	23·53	−0·07	14	+ 8 31 28·61	28·18	+0·43
β Aquilæ	6	19 48 52·66	52·74	−0·08	9	+ 6 4 53·80	53·64	+0·16
1 α Capricor.	2	20 10 23·05	23·15	−0·10	1	−12 54 38·96	38·76	−0·20
2 α Capricor.	5	20 10 46·99	47·10	−0·11	6	−12 56 54·64	55·15	+0·51
α Cygni	4	20 36 57·89	57·98	−0·09	12	+44 48 48·60	48·31	+0·29
61¹ Cygni	..	..	..	..	1	+38 6 24·07	24·46	−0·39
α Cephei	4	21 15 26·85	27·08	−0·23	19	+62 1 52·27	50·89	+1·38
β Cephei	..	..	..	..	11	+69 59 9·32	8·03	+1·29
α Aquarii	6	21 59 3·26	3·32	−0·06	5	− 0 57 18·48	18·14	−0 34
α Piscis aust.	10	22 50 24·31	24·41	−0·10	9	−30 18 57·73	56·83	−0·90
α Pegasi	2	22 58 14·18	14·24	−0·06	6	+14 30 4·18	4·75	−0·57
[ι Piscium]	2	23 33 12·70	12·89	−0·19	2	+ 4 54 59·76	59·13	+0·63
[ω Piscium]	3	23 52 35·01	35·16	−0·15	2	+ 6 8 18·81	17·21	+1·60
α Ursæ Min.	59	1 10 56·80	56·93	−0·13	111	+88 36 23	39·44	−0·21
ℓ Ursæ Min.	10	18 14 36·15	35·86	+0·29	48	+86 36 19·96	19·04	+0·92

The Origin of Lunar Volcanoes. By W. Mattieu Williams, Esq.

Many theoretical efforts, some of considerable violence, have been made to reconcile the supposed physical contradiction presented by the great magnitude and area of former volcanic activity of the Moon, and the present absence of water on its surface. So long as we accept the generally received belief that water is a necessary agent in the evolution of volcanic forces, the difficulties presented by the lunar surface are rather increased than diminished by further examination and speculation.

We know that the lava, scoriæ, dust, and other products of volcanic action on this earth are mainly composed of mixed silicates—those of alumina and lime preponderating. When we consider that the solid crust of the Earth is chiefly composed of silicic acid, and of basic oxides and carbonates which combine with silicic acid when heated, a natural necessity for such a composition of volcanic products becomes evident.

If the Moon is composed of similar materials to those of the Earth, the fusion of its crust must produce similar compounds, as they are formed independently of any atmospheric or aqueous agency.

This being the case, the phenomena presented by the cooling of fused masses of mixed silicates in the absence of water become very interesting. Opportunities of studying such phenomena are offered at our great iron-works, where fused masses of iron cinder, composed mainly of mixed silicates, are continually to be seen in the process of cooling under a variety of circumstances.

I have watched the cooling of such masses very frequently, and have seen abundant displays of miniature volcanic phenomena, especially marked where the cooling has occurred under conditions most nearly resembling those of a gradually cooling planet or satellite ; that is, when the fused cinder has been enclosed by a solid resisting and contracting crust.

The most remarkable that I have seen are those presented by the cooling of the "tap cinder" from puddling furnaces. This, as it flows from the furnace, is received in stout iron boxes ("cinder-bogies") of circular or rectangular horizontal section. The following phenomena are usually observable on the cooling of the fused cinder in a circular bogie.

First a thin solid crust forms on the red-hot surface. This speedily cools sufficiently to blacken. If pierced by a slight thrust from an iron rod, the red-hot matter within is seen to be in a state of seething activity, and a considerable quantity exudes from the opening. If a bogie filled with fused cinder is left undisturbed, a veritable spontaneous volcanic eruption takes place through some portion, generally near the centre, of the solid crust. In some cases, this eruption is sufficiently violent to eject small spurts of molten cinder to a height equal to four or five diameters of the whole mass.

The crust once broken, a regular crater is rapidly formed, and

miniature streams of lava continue to pour from it; sometimes slowly and regularly, occasionally with jerks and spurts due to the bursting of bubbles of gas. The accumulation of these lava-streams forms a regular cone, the height of which goes on increasing. I have seen a bogie about 10 or 12 inches in diameter, and 9 or 10 inches deep, thus surmounted by a cone above 5 inches high, with a base equal to the whole diameter of the bogie. These cones and craters could be but little improved by a modeller desiring to represent a typical volcano in miniature.

Similar craters and cones are formed on the surface of cinder which is not confined by the sides of the bogie. I have seen them beautifully displayed on the "running-out beds" of refinery furnaces. These, when filled, form a small lake of molten iron covered with a layer of cinder. This cinder first skins over, as in the bogies, then small crevasses form in this crust, and through these the fused cinder oozes from below. The outflow from this chasm soon becomes localised, so as to form a single crater, or a small chain of craters; these gradually develope into cones by the accumulation of outflowing lava, so that when the whole mass has solidified, it is covered more or less thickly with a number of such hillocks. These, however, are much smaller than in the former case; reaching to only one or two inches in height, with a proportionate base. It is evident that the dimensions of these miniature volcanoes are determined mainly by the depth of the molten matter from which they are formed. In the case of the bogies, they are exaggerated by the overpowering resistance of the solid iron bottom and sides, which force all the exudation in the one direction of least resistance, viz. towards the centre of the thin upper crust, and thus a single crater and a single cone of the large relative dimensions above described are commonly formed. The magnitude and perfection of these miniature volcanoes vary considerably with the quality of the pig-iron and the treatment it has received, and the difference appears to depend upon the evolution of gases, such as carbonic oxide, volatile chlorides, fluorides, &c. I mention the fluorides particularly, having been recently engaged in making some experiments on Mr. Henderson's process for refining pig-iron, by exposing it when fused to the action of a mixture of fluoride of calcium and oxides of iron, alumina, manganese, &c. The cinder separated from this iron displayed the phenomena above described very remarkably, and jets of yellowish flame were thrown up from the craters while the lava was flowing. The flame was succeeded by dense white vapours as the temperature of the cinder lowered, and a deposit of snow-like, flocculent crystals was left upon and around the mouth or crater of each cone. The miniature representation of cosmical eruptions was thus rendered still more striking, even to the white deposit of the haloid salts which Palmieri has described as remaining after the recent eruption of Vesuvius.

The gases thus evolved have not yet been analytically examined, and the details of the powerful reactions displayed in this

process still demand further study, but there can be no doubt that the combination of silicic acid with the base of the fluor spar is the fundamental reaction to which the evolution of the volatile fluorides, &c., is mainly due.

A corresponding evolution of gases takes place in cosmical volcanic action, whenever silicic acid is fused in contact with limestone or other carbonate, and a still closer analogy is presented by the fusion of silicates in contact with chlorides and oxides, in the absence of water. If the composition of the Moon is similar to that of the Earth, chlorides of sodium, &c., must form an important part of its solid crust ; they should correspond in quantity to the great deposit of such salts that would be left behind if the ocean of the Earth were evaporated to dryness. The only assumptions demanded in applying these facts to the explanation of the surface configuration of the Moon are, 1st, that our satellite resembles its primary in chemical composition ; 2d, that it has cooled down from a state of fusion ; and 3d, that the magnitude of the eruptions, due to such fusion and cooling, must bear some relation to the quantity of matter in action.

The first and second are so commonly made and understood, that I need not here repeat the well-known arguments upon which they are supported, but may remark that the facts above described afford new and weighty evidence in their favour.

If the correspondence between the form of a freely suspended and rotating drop of liquid and that of a planet or satellite is accepted as evidence of the exertion of the same forces of cohesion, &c., on both, the correspondence between the configuration of the lunar surface, and that of small quantities of fused and freely cooled earth-crust matter, should at least afford material support to the otherwise indicated inference, that the materials of the Moon's crust are similar to those of the Earth's, and that they have been cooled from a state of fusion.

I think I may safely generalize to the extent of saying, that no considerable mass of fused earthy silicates can cool down under circumstances of free radiation without first forming a heated solid crust, which by further radiation, cooling, and contraction, will assume a surface configuration resembling more or less closely that of the Moon. Evidence of this is afforded by a survey of the spoil-banks of blast furnaces, where thousands of blocks of cinder are heaped together, all of which will be found to have their upper surfaces (that were freely exposed when cooling) corrugated with radiating miniature lava streams, that have flowed from one or more craters or openings that have been formed in the manner above described.

The third assumption will, I think, be at once admitted, inasmuch as it is but the expression of a physical necessity.

According to this, the Earth, if it has cooled as the Moon is supposed to have done, should have displayed corresponding irregularities, and generally, the magnitude of mountains of solidified planets and satellites should be on a scale proportionate to their

whole mass. In comparing the mountains of the Moon and *Mercury* with those of the Earth, a large error is commonly made by taking the customary measurements of terrestrial mountain-heights from the sea level. As those portions of the Earth which rise above the waters are but its upper mountain slopes, and the ocean bottom forms its lower plains and valleys, we must add the greatest ocean depths to our customary measurements, in order to state the full height of what remains of the original mountains of the Earth. As all the stratified rocks have been formed by the wearing down of the original upper slopes and summits, we cannot expect to be able to recognise the original skeleton form of our water-washed globe.

If my calculation of the atmosphere of *Mercury* is correct, viz. that its pressure is equal to about one-seventh of the Earth's, or $4\frac{1}{4}$ inches of *Mercury*, there can be no liquid water on that planet, excepting perhaps over a small amount of circumpolar area, and during the extremes of its aphelion winter. Thus the irregularities of the terminator, indicating mountain elevations calculated to reach $\frac{1}{253}$ of the diameter of the planet, are quite in accordance with the above-stated theoretical considerations.

There is one peculiar feature presented by the cones of the cooling cinder, which is especially interesting. The flow of fused cinder from the little crater is at first copious and continuous; then it diminishes and becomes alternating, by a rising and falling of the fused mass within the cone. Ultimately the flow ceases, and then the inner liquid sinks, more or less, below the level of the orifice. In some cases, where much gas is evolved, this sinking is so considerable as to leave the cone as a mere hollow shell; the inner liquid having settled down and solidified with a flat or slightly rounded surface, at about the level of the base of the cone, or even lower. These hollow cones were remarkably displayed in some of the cinder of the Henderson iron, and their formation was obviously promoted by the abundant evolution of gas.

If such hollow cones were formed by the cooling of a mass like that of the Moon, they would ultimately and gradually subside by their own weight. But how would they yield? Obviously by a gradual hinge-like bending at the base towards the axis of the cone. This would occur with or without fracture, according to the degree of viscosity of the crust, and the amount of inclination. But the sides of the hollow-cone shell, in falling towards the axis, would be crushing into smaller circumferences. What would result from this? I think it must be the formation of fissures, extending, for the most part, radially from the crater towards the base, and a crumpling up of the shell of the cone by foldings in the same direction. Am I venturing too far in suggesting, that in this manner may have been formed the mysterious rays and rills that extend so abundantly from several of the lunar craters?

The upturned edges or walls of the broken crust, and the chasms necessarily gaping between them, appear to satisfy the peculiar phenomena of reflection which these rays present.

These edges of the fractured crust would lean towards each other, and form angular chasms ; while the foldings of the crust itself would form long concave troughs, extending radially from the crater.

These, when illuminated by rays falling upon them in the direction of the line of vision, would reflect more light towards the spectator than would the general convex lunar surface, and thus would become especially visible at the full Moon.

Such foldings and fractures would occur after the subsidence and solidification of the lava-forming liquid — that is, when the formation of new craters had ceased in any given region ; hence they would extend across the minor lateral craters formed by outbursts from the sides of the main cone, in the manner actually observed.

The fact that the bottoms of the great walled craters of the Moon are generally lower than the surrounding plains must not be forgotten in connexion with this explanation.

I will not venture further with the speculations suggested by the above-described resemblances, as my knowledge of the details of the telescopic appearances of the Moon is but second-hand. I have little doubt, however, that observers who have the privilege of direct familiarity with such details, will find that the phenomena presented by the cooling of iron cinder, or other fused silicates, are worthy of further and more careful study.

On an Instance of Abnormal Refraction. By Rev. J. Slatter.

1873, January, $7^d\ 14^h$, I observed a singular instance of abnormal refraction. *Vega* visible a few degrees above the horizon in the N.N.W. was elongated into a bar of light about 4′ in length. I thought at first it was a comet, as it rather reminded me of the Great Comet of 1861, when I became sure, by comparison of neighbouring stars, that it was *Vega*. I supposed it was some moisture on the eye, but continued looking confirmed that the appearance was due to refraction, and the reflexion in the river exactly corresponded to the true image in the sky.

I should judge that the inclination of the elongation was about 60° to the horizon. I have seen the Moon elongated into a bar, but this was vertical.

Streatley Vicarage, Berks,
Feb. 4, 1873.

Ephemeris for Physical Observations of the Moon.

By A. Marth, Esq.

(Continued from page 156.)

Selenographical Colong. and Latitude of the Point on the Moon's Surface, which has the

Gr. Midnight. 1873.	Sun's Centre in the Zenith. Colong.	Sun's Centre in the Zenith. Lat.	Earth's Centre in the Zenith. Colong.	Earth's Centre in the Zenith. Lat.	Geocentric Libration Amount.	Geocentric Libration Direction.	
	°	′ °	°	°	°	°	
April 29	302·97	−0·41	84·48	−2·75	6·17	26·5	s. p. quadrant.
30	315·20	·39	84·83	4·07	6·58	38·3	„
May 1	327·42	·37	85·49	5·16	6·85	49·0	„
2	339·64	·34	86·40	5·98	6·98	59·1	„
3	351·86	·32	87·50	6·53	6·99	69·1	„
4	4·07	−0·29	88·72	−6·79	6·91	79·4	„
5	16·27	·26	89·97	6·75	6·75	89·8	s. p.
6	28·47	·24	91·21	6·43	6·54	79·4	s. f.
7	40·67	·21	92·35	5·82	6·28	68·1	„
8	52·86	·18	93·35	4·94	5·97	55·9	„
9	65·04	·15	94·16	3·83	5·65	42·7	„
10	77·22	·12	94·74	2·48	5·35	27·7	„
11	89·40	−0·08	95·07	−1·02	5·17	11·4	s. f.
12	101·59	·05	95·11	+0·55	5·14	6·2	n. f.
13	113·77	− ·02	94·84	2·11	5·28	23·6	„
14	125·95	+ ·01	94·36	3·58	5·64	39·5	„
15	138·14	+0·04	93·70	4·87	6·11	52·9	„
16	150·34	+0·07	92·89	5·88	6·55	63·9	„
17	162·54	+0·10	91·95	6·54	6·82	73·5	„
18	174·75	+0·12	90·96	+6·81	6·88	82·5	n. f.
19	186·96	·15	89·87	6·66	6·66	88·9	n. p.
20	199·19	·18	88·79	6·11	6·23	78·8	„
21	211·42	+0·20	87·76	+5·18	5·64	66·6	„
29	309·37	+0·39	86·51	−5·73	6·71	58·8	s. p.
30	321·61	·41	87·42	6·37	6·87	68·0	„
31	333·84	·44	88·53	6·72	6·88	77·7	„
June 1	346·07	+0·46	89·71	−6·77	6·78	87·6	s. p.
2	358·30	·49	90·97	6·54	6·61	81·6	s. f.
3	10·52	·51	92·22	6·01	6·41	69·8	„
4	22·73	·54	93·37	5·23	6·22	57·3	„
5	34·93	·57	94·36	4·20	6·05	44·0	„
6	47·13	·60	95·11	2·95	5·90	30·1	„
7	59·33	·63	95·57	−1·52	5·77	15·3	s. f.

Gr. Midnight. 1873.	Sun's Centre in the Zenith. Colong.	Lat.	Earth's Centre in the Zenith. Colong.	Lat.	Geocentric Libration. Amount.	Direction.
	°	°	°	°	°	
June 8	71·53	+0·65	95·70	+0·02	5·70	0·2 n. f.
9	83·72	·68	95·47	1·59	5·70	16·2 „
10	95·91	·71	94·90	3·12	5·81	32·5 „
11	108·09	·74	94·00	4·48	6·00	48·3 „
12	120·28	·76	92·87	5·59	6·20	64·5 „
13	132·48	·79	91·57	6·35	6·54	76·2 „
14	144·68	·81	90·22	6·71	6·71	88·1 n. f.
15	156·89	+0·84	88·92	+6·65	6·74	80·8 n. p.
16	169·10	·86	87·73	6·17	6·57	69·9 „
17	181·33	·88	86·73	5·32	6·24	58·5 „
18	193·55	·90	85·95	4·16	5·80	45·8 „
19	205·78	·92	85·39	2·78	5·38	31·1 „
20	218·02	+0·94	85·05	+1·26	5·11	14·3 n. p.
28	316·02	+1·08	89·33	−6·67	6·70	84·3 s. p.
29	328·26	1·10	90·57	6·50	6·52	85·0 s. f.
30	340·50	1·12	91·86	6·06	6·34	73·0 „
July 1	352·73	1·14	93·13	5·35	6·20	59·8 „
2	4·95	1·16	94·33	4·40	6·17	45·5 „
3	17·17	1·18	95·36	3·24	6·26	31·2 „
4	29·39	1·20	96·14	1·91	6·43	17·3 „
5	41·59	1·21	96·58	−0·45	6·60	3·9 s. f.
6	53·79	+1·23	96·63	+1·08	6·72	9·3 n. f.
7	65·99	1·25	96·25	2·60	6·77	22·6 „
8	78·18	1·27	95·41	4·04	6·75	36·8 „
9	90·37	1·29	94·17	5·19	6·65	51·3 „
10	102·56	1·31	92·61	6·07	6·61	66·8 „
11	114·75	1·32	90·86	6·54	6·60	82·5 n. f.
12	126·95	1·34	89·09	6·57	6·63	82·1 n. p.
13	139·15	+1·35	87·45	+6·16	6·66	67·6 „
14	151·35	1·36	86·05	5·36	6·65	53·7 „
15	163·56	1·37	84·96	4·24	6·58	40·2 „
16	175·78	1·38	84·23	2·89	6·45	26·7 „
17	180·00	1·39	83·83	+1·40	6·33	12·8 n. p.
18	200·23	1·40	83·74	−0·13	6·26	1·2 s. p.
19	212·47	1·41	83·91	−1·62	6·31	15·2 „
20	224·71	+1·41	84·30	−3·00	6·44	27·8 s. p.

Ephemeris for Physical Observations of Mars' Opposition of 1873.

By A. Marth, Esq.

1873. Gr. Noon.	Areographical Western Longitude of Centre of Mars' Disk.	Difference.	Areographical Latid. of Centre of Mars' Disk.	Angle of Posit. of Mars' Axis.	Diameter.	Defect of Illumination. Prec. Limb	North Limb.	Greatest Amount.	Posit.
	°	° °	°	°	″	″	″	″	°
April 1	266·7		+14·44	41·05	13·93	0·28	0·03	0·30	286·6
		1752 + 3·5							
6	222·2		14·86	41·14	14·55	0·23	0·02	·24	285·5
		„ + 3·5							
11	177·7		15·43	41·15	15·13	·16	0·01	·17	284·4
		„ + 3·8							
16	133·5		16·13	41·15	15·64	·09	0·00	·10	283·0
		„ + 4·0							
21	89·5		16·92	41·11	16·06	0·03	..	·03	280·0
		„ + 4·1							
26	45·6		17·78	41·04	16·36	foll. l.	south l.	·00	
		„ + 4·2							
May 1	1·8		18·67	40·92	16·53	0·01	0·00	·01	118·3
		„ + 4·2							
6	318·0		19·54	40·75	16·55	·06	·01	·07	112·6
		„ + 4·1							
11	274·1		20·35	40·56	16·45	·15	·02	·17	111·3
		„ + 3·8							
16	229·9		21·08	40·37	16·22	·27	·04	·31	110·8
		„ + 3·7							
21	185·6		21·72	40·16	15·89	·41	·06	·46	110·6
		„ + 3·3							
26	140·9		22·21	40·06	15·48	·55	·08	·62	110·5
		„ + 2·9							
31	95·8		22·59	39·98	15·02	·67	·11	·77	110·5
		„ + 2·6							
June 5	50·4		22·85	39·96	14·52	·82	·13	0·91	110·4
		„ + 2·3							
10	4·7		22·98	40·01	14·02	0·94	·16	1·04	110·4
		„ + 2·0							
15	318·7		22·99	40·13	13·51	1·03	·18	1·14	110·3
		„ + 1·6							
20	272·3		+22·88	40·30	13·01	1·11	0·19	1·22	110·2

The difference 1752° of the successive values in the column of longitudes corresponds to an hourly increase of 14°·6. To find the areographical longitude of the centre of the disk for the hour of observing, the following values are to be added to the next preceding value given in the table (subtracting 360°, when the sum is larger), on the first, second, third, fourth, and fifth day.

	°	°	°	°	°
At 8h Gr.M.T.	116·8	107·2	97·6	88·0	78·4
9	131·4	121·8	112·2	102·6	93·0
10	146·0	136·4	126·8	117·2	107·6
11	160·6	151·0	141·4	131·8	122·2
12	175·2	165·6	156·0	146·4	136·8
13	189·8	180·2	170·6	161·0	151·4
14	204·4	194·8	185·2	175·6	166·0

and some further allowance must be made for the surplus of the difference over 1752°.

The ephemeris is founded upon the same elements as the ephemeris for the opposition of 1869, given in the *Monthly Notices*, and that for the opposition of 1871, given in the *Astronomical Register*.

Note on the want of Observations of Eclipses of Jupiter's First Satellite from 1868 *to* 1872. By Sir G. B. Airy.

Professor Newcomb, of the United States Naval Observatory, Washington, is desirous of receiving observations of eclipses of *Jupiter's* First Satellite from 1868 to 1872, accompanied with a statement, for each observatory, of the authority for time, and of the aperture of the telescope employed in each observation. The observations can be transmitted either directly to Professor Newcomb or to the Astronomer Royal.

Royal Observatory, Greenwich,
1873, *March* 12.

Discovery of Minor Planet (129). By Dr. H. C. F. Peters.

(*From a Letter to the Astronomer Royal.*)

Of a new planet, discovered here on February 5th, telegraphic notice was sent you through the Smithsonian Institution. I am happy thus to have been fortunate enough to inaugurate the use of the liberal concession made to science by the Atlantic Cable Companies for the communication, free of charge, of astronomical discoveries.

The weather unusual for the season of the year, has favoured me so as to obtain hitherto the following observations:

1873.	Ham Coll. Mean Time.	α (129)	δ (129)	No. of Comp.	Log (p'')	Log ($p''\Delta$)
		h m s	° ′ ″			
Feb. 5	15 21 53	9 16 32·94	+ 15 31 50·8	16	0·702	0·670
6	11 34 52	9 15 49·97	15 38 24·1	10	9·969	0·613
7	12 23 42	9 14 57·18	15 46 30·1	10	9·820	0·610
9	11 26 23	9 13 17·03	+ 16 1 43·3	10	9·880	0·606

The planet's magnitude I would rather estimate at 9·5.

Clinton, New York,
1873, *Feb.* 11.

Transit of Venus in 1874.

In reference to the observation of the Transit of *Venus* in 1874, I have much pleasure in communicating to the Society an extract from a letter lately received from M. D'Abbadie, which tends to remove an important difficulty. G. B. Airy.

"It is settled that Mons. Fleurien, a naval officer well known for his ability in common astronomical observations, will observe the passage of *Venus* at the Marquesas Island.

"Antoine D'Abbadie.

"*Paris,* 1873, *March* 28."

Errata in Annual Report.

Page 200, line 29, *for* fifty-three, *read* fifty-five.
211, 9, *for* seventy-fifth, *read* sixty-fifth.